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CONTENTS

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Psychological Bulletin

TESTING FOR PSYCHOMOTOR ABILITIES BY MEANS OF APPARATUS TESTS

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One area of aptitude testing which has received relatively little development is that of psychomotor performance. Yet, it appears that a number of job specialties involve sizable components of motor activity. While the field of motor skills *learning* is a thriving basic research area, there has been only a limited attempt to examine the problem of motor skills from the point of view of *aptitude testing*. Moreover, basic research in this aptitude area has been quite limited compared with research in other aptitude areas. Even in the Air Force research program, which probably represents the most ambitious program of psychomotor test development ever attempted, efforts at a basic research level have lagged far behind test developments in the other fields.

A thorough picture of the Air Force program of research in perceptual-motor skill testing during World War II has been presented by Melton (**18**). The present paper will touch only briefly on some of the problems and findings discussed there. Although previous research in this area will be explored, primary emphasis will be given to a discussion of some of the fundamental problems and issues involved in motor skills testing research and to implications for possible future test development and research. A major section is devoted to a review of previous factor analysis studies in the area of psychomotor performance.

It will be useful at the outset to distinguish between certain concepts in this area about which there has often been some confusion.

In the field of motor ability testing, the subject is generally presented with some standardized task in which he must respond by means of certain muscular activities rather than by some verbal means. The primary interest is in individual differences with respect to these *response aspects* of the subject's behavior in the task situation. The term "motor" refers primarily to the muscular activities which can be measured. However, it is true that in these tasks the subject is responding to some simple or complex stimulus situation; hence, the term "perceptual-motor" or "psychomotor" is often used. The distinction between perceptual and motor skills is a somewhat arbitrary one, each class of skills being represented in varying degrees in the performance of different tasks. There is still no general agreement concerning whether it is more desirable to con-

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ceive of tasks as varying along a single dimension from perceptual to motor or as varying along two dimensions, complexity of stimulus and complexity of motor activity required. The distinction between perceptual skills and motor skills in this paper is based only upon the relative emphasis given to certain factors in the performance situation. The concern will be with tests measuring individual differences in the primarily motor aspects of the test situation. Thus, the *making* rather than the *selecting* of the response will be the important consideration, although the kind or intensity of motor activity may be influenced in varying degrees by the nature of the stimulating conditions. The use of the term "psychomotor" is a recognition of this latter fact. For example, one may be interested in measuring the subject's coordination or speed of certain motor responses with his perception of certain cues. In all cases, the primary interest of this paper is in the motor activity involved.

Historical Framework of Psychomotor Test Development

The use of tests of psychomotor functions is as old as the history of individual differences measurement itself, dating back to the work of Galton and James McKeen Cattell in the 1890's. At about the same time, Munsterburg, Jastrow, Kraepelin, and others were including simple motor skills tests in their investigations of "mental" ability. Complex mental processes were believed to be best understood by analyzing them into their elementary components, usually of a sensory-motor nature. However, starting with the work of Binet and Henri, and Ebbinghaus, the trend was toward the development of more complex tests of "intelligence." Although certain motor tasks were included in the early Binet scales, the development of "intelligence" tests and motor skill tests went their separate ways. Intelligence testing, aided by group testing development in World War I, attained undreamed-of proportions. When many exaggerated initial expectations became unfilled, there was a shift in emphasis from the exclusive use of intelligence tests to the measurement of *special* aptitudes, especially with adults and older adolescents. Special combinations of tests in different aptitude areas into test batteries, were now used for classification and selection purposes. With the possible exceptions of certain dexterity tests, psychomotor tests were almost totally excluded from these batteries. Perhaps the most significant development in the trend away from complex general intelligence tests was the refinement and application of factor-analysis techniques. New insights into more basic and independent categories of ability had emerged, and tests could be constructed to sample, in relatively pure form, each separate ability area. The fruitfulness of this development was even further enhanced when the factor composition of jobs and criteria could be included in the factor analysis (**13**, **19**).

Test development in the field of motor skills has not exactly paralleled this development of tests in the more "intellectual" areas. Early tests of motor skills were of the simplest kind. Much of the research on these tests was confined to the laboratory situation and was designed primarily to investigate such things as generality versus specificity of simple motor abilities, and also the factors underlying individual differences in certain motor skills. Thus, studies by Robert Seashore and his co-workers (**27**, **30**, **34**), Reymert (**22**), and Camp-

bell (**5**) indicate that in fine motor skills the sense employed is of moderate significance, the musculature employed is of very slight significance, and the pattern of movement involved is likely to be the most important factor. However, a basic classification of these patterns is still awaited (this is further discussed in a later section). Moreover, the investigators largely concluded that motor factors are relatively few and very narrow in scope (**4**, **20**, **21**, **25**, **27**, **28**). The studies have generally shown simple motor skill tests to correlate very low with each other.

Under Seashore's influence, a number of simple motor skill tasks were standardized and tried out on on-the-job criteria. The Stanford Motor Skills Unit (**24**) was an example of these tests. Several validity studies using these tests were subsequently made. Examples are those of Harney (described by Seashore [**28**]) in predicting high school shopwork grades, by Walker and Adams (**40**) in predicting typewriter skill, and by S. H. Seashore (**33**) in predicting success in winding-machine apprentices in a knitting mill. The validities in all cases were insignificant. Moreover, other studies (**25**) showed low relationships between total training that people had in certain manual skills and many tests of fine motor skills. There have been, however, some notable exceptions. These include a study by Spaeth and Dunham (**35**), who reported a correlation of .61 between a test for precision in thrusting a stylus at a graded series of holes and target rifle shooting for 73 Army men. Seashore and Adams (**29**) also found that five simple steadiness tests distinguished sharply between a university rifle team and 50 unselected ROTC students. Humphreys, Buxton, and Taylor (**17**) corroborated these findings in a similar study. They found a correlation of .77 between similar steadiness tests and rifle marksmanship.

Studies of finger and manual dexterity tests have also on occasion shown some validity for watchmakers, electrical fixture and radio assemblers, coil winders, packers and wrappers, and certain kinds of machine operators. Summaries of such validity studies involving simple dexterity tests can be found in Tiffin (**39**) and Super (**36**).

The assumption underlying simple motor skill test development was that it should be possible to develop a battery of simple motor tests which would indicate likelihood of success in a more complex motor skill. So strongly was this belief held that failures in prediction have often been attributed to faulty techniques such as lack of reliability of the measures used. In some cases this was probably justifiable, especially since in most of the studies the reliabilities were not determined, or, at least, not published. However, in other studies in which reliabilities were known (.80 to .90) the conclusions were the same.

Complex tests of motor skills were not developed to any extent until the World War II Air Force research program. That these kinds of tests had substantial validity was demonstrated. Here, paradoxically, were two trends going in opposite directions. In the paper-and-pencil test areas, test development was increasingly being aimed at simpler tests of one factor at a time. Factor analysis of printed tests was continuously utilized to obtain fewer tests, as pure factorially as possible. On the other hand, psychomotor tests were becoming more complex using increasingly complicated apparatus. These apparatus tests, although contributing considerable validity, correlated quite highly with each other. More-

over, there was no concerted effort, as was made in the paper-and-pencil area, to use factor analysis data in developing and organizing apparatus tests along new and relatively independent dimensions.

Some Characteristics of Apparatus Testing

Recent aptitude testing, in the area of perceptual-motor ability, to a great extent has consisted of individual apparatus tests. The apparatus may vary in complexity from simple pegboards to complicated mechanical or electronic contrivances. The assumption underlying the development and use of apparatus tests is that certain kinds of abilities can best be measured by these performance tests as contrasted with paper-and-pencil tests. Individual apparatus tests often appear necessary whenever the primary interest is in the motor aspect of the subject's responses. Such functions as perceptual-motor coordination, smoothness of control movement, speed of discriminative reactions, appropriateness of control movement, responsiveness to kinesthetic cues, and motor control under stress conditions are examples of skills for which apparatus tests seem more suited than printed tests. Whenever the interest is in the *making* rather than the *selecting* of the response, some instrument is usually needed to provide the cues to be responded to, the means of response, and the means for recording the speed, precision, or other relevant features of the response.

Very detailed descriptions of apparatus tests developed in the Air Force Classification Program may be found in Melton (**18**). Some of the better-known tests described include the Complex Coordination, Two-Hand Coordination, Rotary Pursuit, Rudder Control, Discrimination Reaction Time, Aiming Stress, and Finger Dexterity Tests.

In most cases, each test consists of a task unit (which the subject operates) and a control unit which contains the timing apparatus, counters which record the scores, and switches which the examiner uses to control the testing period. There may be from two to ten task units (depending on size) connected to one control unit.

Apparatus Test Problems

Melton (**18**) has summarized the problems that accompany the use of apparatus tests and the steps that were taken to solve them in the Air Force program. Some of these difficulties are sketched only briefly here.

First, such tests are expensive to build and expensive to maintain. Moreover, since a test can be given to only a few subjects at a time, apparatus tests are administratively more expensive than tests which can be given to groups of a hundred subjects by only three or four administrators. The task of maintaining uniformity of testing conditions for each man tested is also increased with apparatus tests. This includes lack of uniformity owing to examiner differences and to apparatus differences. Examiner differences can be reduced by increased training. There is also some evidence (**2**) that the examiner is not a very potent source of variability, at least in situations where the possibility of timing and recording errors is minimized. Maintaining uniformity of the apparatus from person to person is a more serious problem. This includes maintaining comparability of scores within the same piece of apparatus as well as differences between several copies of the same apparatus. In the Air Force program, it was found necessary to

build apparatus to high standards of precision and to subject them to a rigorous program of preventive maintenance and of calibration. In addition, a system of statistical control to check on apparatus differences was also found necessary.

Several other apparatus testing problems have received consideration. Studies investigating whether the number of subjects tested at one time influenced individual test scores have generally shown little effect of this "social-interaction" variable (**1**, **18**).

In another study, it was found that different orders of presentation of different apparatus tests in a battery affected scores significantly in the case of only one test (Aiming Stress). However, it would be hazardous to extrapolate to other test batteries from this generally negative finding. At any rate, some constant order of presentation of each test relative to other tests in a battery would seem to be necessary.

Apparatus tests also present special problems in the determination of intratest and test-retest reliability coefficients, since performance of subjects in the tests almost invariably shows improvement with practice. The usual procedure is the intratest correlation where the total test period is divided into trials and odd versus even trials are correlated. The chief value of these coefficients is in their use relative to the interpretation of intercorrelations with other tests in the battery given at the same time.

Thorndike (**37**) has pointed out an additional difficulty encountered in using apparatus tests. This is the difficulty of assembling test records fast enough that validation data can become available upon an adequate sample within a reasonable time. Because only a few pilot copies of such tests are usually available for such experimental use, testing may have to continue over long periods of time in order to accumulate sufficient samples for whom criterion data will become available in different job specialties. Adding the time necessary for the criterion data to mature, it can be seen that the validation program for apparatus tests can be a slow-moving affair. The present Air Force program has partially solved this problem by the use of mobile trailer units containing copies of the tests. These units can be moved around the country to different Air Force bases whenever desirable criterion groups are available.

Although it is important to take into account these limitations of apparatus tests, and to hold the controllable sources of error to a minimum, the value of the tests must ultimately be assessed in terms of the unique contribution they can make to the over-all prediction of the criteria. That "something" unique (not tested by other tests) has been added to the Air Force batteries by apparatus tests has been repeatedly demonstrated (**13**, **18**). For example, the addition of six psychomotor tests to fourteen printed tests raised the multiple correlation of the battery with the pilot criterion from approximately .50 to .70.

Factor analyses of the complete battery consistently revealed three psychomotor factors measured by the apparatus tests included. A factor identified as "Psychomotor Coordination" was found to be quite general. It was found in tests requiring small-muscle adjustments (finger dexterity tests) as well as tests requiring large-muscle adjustments (movements of arms, torso, and legs). A second factor was tentatively named "Psychomotor Precision" because it was involved in tests requiring accurate

manipulations under speed conditions (discrimination reaction time, finger dexterity). A third factor was tentatively called "Psychomotor Speed" because it was primarily involved in tests requiring sheer speed of marking an answer sheet. An additional factor hypothesized was described as a "Kinesthetic-Motor" factor measured almost uniquely by the Rudder Control Test. The analyses also indicated substantial loadings of these factors in the different criteria. The factor analyses also revealed, however, that a few paper-and-pencil tests in the battery sampled at least some factors measured by apparatus tests. Moreover, the indication from the test communalities was that the variance of the psychomotor tests in general is not as well accounted for by the present factors as that of the printed tests.

Implications of these findings and more complete descriptions of factor analysis studies of motor skills tests are presented in later sections.

Factor Analyses of Psychomotor Tests

It seems apparent that some extensive dimensional analysis of motor abilities is necessary. It is also proposed that factor analysis techniques can aid greatly in this respect. Although factor analysis methods have been employed in this area on a limited scale, they have been performed on either a limited number of simple motor tasks or on a limited number of complex tasks. A factor analysis of a much wider variety of motor tasks which could reveal a more basic classification of factors primarily important in the performance of the tasks would seem desirable. This would not only help bring order into the field of psychomotor testing but would provide a framework for research in general in the area of motor skills. Such a methodological approach could lead to a clearer delineation of unique psychomotor factors and a better understanding of the variables that contribute to the validity of both simple and complex tasks.

In this section, a summary will be given of some *previous* factor studies that have been made. This may provide additional background for needs in this area and may also provide suggestions for the kinds of tasks to be included in future factor analysis batteries.

Difficulties in Comparing Different Factor Studies

There are definite problems and limitations which one encounters in comparing different factor analysis studies. A major difficulty is the different interpretation given to factors by different researchers. This is sometimes only a question of semantics, but in at least a few cases two investigators might give entirely different meaning to factors. Also, the interpretations given factors are often based on much too limited evidence.

There is also the difficulty of comparing factors derived by different factor methods. This paper will be confined to studies using variations of Thurstone's centroid procedure. Investigators may still differ in (*a*) their criteria of when the solution is reached, (*b*) whether they favor oblique or orthogonal solution, and (*c*) whether they rotate "blindly" or with use of certain "hypotheses." However, results generally show quite close agreement between these different emphases in rotation.

Another major problem is the lack of identity of tests in the different batteries factor analyzed. Thus, factors based on different sets of tests may be named the same but that they

are operationally the same may not have been demonstrated.

Then there is the matter of sampling errors. No satisfactory measure of the standard error of a factor loading has been determined. Thus small loadings may not indicate that the test is measuring a given factor. However, if these small loadings repeatedly show up on replications of the factor analyses, one might have considerable confidence in their significance.

The summary of previously isolated psychomotor factors to be presented here is not intended to be as detailed as the excellent surveys published by Cattell (**6**) in the field of personality or by French (**9**) in the field of aptitude and achievement testing. However, it is intended to be comprehensive in at least the coverage of previously published studies.

Factors Derived

The studies considered here ranged from factor analyses of very simple motor tests to analyses of complex apparatus test batteries. In general, studies of simple motor skills suggest that if there are underlying factors of motor ability they are relatively narrow and do not extend over a wide range of tests. In more complex tests broader factors appear. Some of the ***nonmotor*** factors which seem most consistently identified in psychomotor test batteries will also be discussed. Of course, these latter factors can only be identified in those few cases where nonmotor tests were included in the total test battery.

Reaction Time. This refers to the speed with which an individual can make a predetermined response, usually of a fairly simple type, to a presented stimulus. This has appeared repeatedly as a separate factor wherever measurement of relatively simple reaction time tests has been included in the analysis. Thurstone (**38**) in his analysis of many tasks loaded with perceptual elements found a factor common only to reaction time to light and reaction time to auditory stimulation. Similarly, Seashore, Buxton, and McCollom (**30**) identified such a factor with loadings on visual jump (subject has to move his hand six inches to the peg) reaction time, auditory simple reaction time, visual simple reaction time, simple horizontal tapping, and auditory jump reaction time. The appearance of this factor emphasizes again that the pattern of movement is of greater significance than the particular sense modality or musculature involved. These studies have involved simple reaction time tasks. There is some evidence that this factor may extend to more complex reaction time tasks. Thus, Seashore, Starman, Kendall, and Helmick (**32**) found correlations of from .63 to .98 among reaction time tasks of varying complexity.

Tapping Ability. This is the speed with which the subject can oscillate either his fingers or his arm. This seems relatively independent of any eye-hand coordination ability. For example, Greene (**12**) found this factor in his analysis of a variety of tasks involving aiming, tapping, and dotting with telegraph keys, fingers, pencils, etc. He found the more eye-hand coordination involved in the task, the smaller became the loadings on the Tapping factor. Thus, tapping with a pencil, making no effort to tap in a particular spot, yielded a high loading on Tapping. Tapping in large circles yielded moderate loadings on Tapping and similar loadings on another factor he named "Aiming." Finally, tapping in small circles where positioning the pencil was difficult yielded practically no

loading on Tapping but high loadings on Aiming. The Aiming factor is discussed later, but it appears that the two factors are usefully considered separate. Tests which Greene found with loadings on this Tapping factor included: dotting in large circles and tapping with either the left or right hand. Guilford in his factor analysis of dexterity tests (reported by Melton [**18**]) identified Tapping as a separate factor. The only two tests with appreciable loadings on this factor were tapping using finger action and tapping using wrist action. Similarly, Wittenborn (**41**) found this factor in his analysis of mechanical ability tests (although he called it "Repetitive Movement"). Seashore, Buxton, and McCollom (**30**) also isolated a Tapping factor in their test battery but they further broke it down into two subfactors. One seemed to involve finger-hand speed in restricted oscillatory movement in one place only. Tasks using tapping keys, a two-finger oscillometer, and short movements had loadings on this factor. The second factor involved forearm and hand speed in oscillatory movements of moderate extent in two planes. This factor was best measured by stylus tapping on two or three plates successively. These two subfactors isolated by Seashore, Buxton, and McCollom may be related to the finger versus manual dexterity distinction discussed later.

Psychomotor Coordination. This factor has appeared in all the Air Force analyses which included apparatus psychomotor tests. It represents either integration of muscular movements or coordination between the eye and muscular movements. It is measurable in the rotary pursuit, complex coordination, two-hand coordination, finger dexterity, aiming stress, and rudder control tasks. It is thus quite general to skeletal musculature and, though common to finer as well as grosser movements, seems best measured by movements of moderate scope. Analyses revealing this factor have been discussed in the Air Force research reports (**13**, **18**), by Dudek (**8**), Michael (**19**), and by Guilford and Zimmerman (**14**).

Zachert and Shibe (**42**), in their factor analysis of the United States Employment Service Battery, which contained no complex apparatus tests, identified a factor as Psychomotor Coordination. The assembling, disassembling, and placing blocks tests had the highest loading on this factor. However, it appears that the factor is the same as what has usually been called a Manual Dexterity factor. With present evidence, it seems safer to consider Manual Dexterity separate from Psychomotor Coordination. Tests of the factor Psychomotor Coordination involve more coordination between muscle groups, are not entirely restricted to arm movements, and do not seem as concerned with speed. It is possible that the Psychomotor Coordination factor may break down into several simple factors if other kinds of less complex tasks were also included in these factor analysis batteries.

Manual Dexterity. This factor involves arm-hand coordination and speed. Tests with loadings on this factor require skillful, controlled arm or hand movements at a rapid rate. The factor appears in analyses of batteries such as the Minnesota and United States Employment Service Tests. Wittenborn (**41**) found this factor in a battery containing many variations of pegboards. This study also indicated that this factor does not depend on the visual sense. Thus, a "pegs" task, where there was a screen in front of the pegs, and a "tactual" task of sorting by touch

revealed loadings on the factor. The United States Employment Service analyses[2,3,4] also showed such a factor common to tasks requiring blocks to be turned over, blocks to be placed in holes, pegs to be moved, turned, inserted, and washers and rivets unassembled. Harrell (**15**), in his analyses of various mechanical and manual ability tests, also isolated such a factor which he calls "Agility." Again, various pinboards, pegboards, simple assembly and disassembly tasks emerged clustered on one factor.

Finger Dexterity. Difficulty arises in distinguishing between what some writers call Manual and others call Finger Dexterity. It appears, however, that Manual Dexterity is the more general factor and may be common to tasks in which an additional factor Finger Dexterity is present. Finger Dexterity involves the rapid manipulation of objects with the fingers. It is distinguished from Manual Dexterity in that it does not include arm motion (although some tasks may require both factors). Thus, placing and turning blocks generally have loadings on Manual Dexterity, but not Finger Dexterity, and pegboards often include both factors. Guilford (reported by Melton [**18**]), in his analysis of dexterity tasks, found five factors to account for the intercorrelations of the 18 tests in the battery. One of these factors he named "Dexterity," and this factor included various pegboards, marking, dotting, simple manipulations, and a finger dexterity test. His analysis did not isolate separate Finger and Manual Dexterity factors. However, many other investigators have isolated a Finger Dexterity factor and it seems useful to handle them separately although they appear related. It is also to be distinguished from Aiming in that accurate eye-hand positioning seems not to be required.

This Finger Dexterity factor has been named in several United States Employment Service test battery analyses.[5] Tests with loadings on it in one analysis included finger dexterity—assembling, and finger dexterity—disassembling (these two tests together with various pegboards also had loadings on Manual Dexterity). In other U.S.E.S. analyses, these two tests, tweezer dexterity, and two small pegboards had loadings on this separate factor in addition to loadings on a Manual Dexterity factor.

Psychomotor Precision. In several Air Force analyses (**13**) a factor named Psychomotor Precision seems similar to Finger Dexterity, although more eye-hand coordination seems involved. The factor was most heavily weighted in psychomotor tests requiring manipulations under speed conditions. It is distinguished from Psychomotor Coordination in that grosser arm motion is not included. Tests most constantly loaded on this factor were a "twisting pegs in a pegboard" task, the Discrimination Reaction Time Test, and in a later analysis, the Rotary Pursuit Test. This cluster has also appeared in the postwar Air Force analyses. The factor requires further investigations with new tests to clear up its status, especially in relation to Finger and Manual Dexterity. Thus, Zachert and Shibe (**42**), in a recent

[2] UNITED STATES EMPLOYMENT SERVICE. Factor analysis studies; report for Group 3. The preliminary factor analysis study. Unpublished manuscript, Washington, D. C.

[3] UNITED STATES EMPLOYMENT SERVICE. Factor analysis studies; report for Group 3. Unpublished manuscript, Washington, D. C.

[4] UNITED STATES EMPLOYMENT SERVICE. Report of factor analysis for Group VII. Unpublished manuscript, Washington, D. C., 1944.

[5] See footnotes 2, 3, and 4.

analysis, named one of their factors "Psychomotor Precision" with an alternate name of "Finger Dexterity." The battery, however, contained no complex apparatus tests, and the tests loaded on this factor (turning blocks, mark-making, placing blocks, disassembling) indicate that Finger or Manual Dexterity would be a more appropriate name on the basis of present evidence.

Although this factor has been named Psychomotor Precision, it seems to involve speed to a great extent. More clearly, this "precision" factor does not refer to the kinds of precision which minimize speed (such as accurate tracing, thrusting a stylus accurately in holes or holding it stationary in holes). Nor does it refer to the various types of postural steadiness involving grosser musculature. Moreover, it does not seem related to the Aiming factor which seems to involve both a series of precise movements *and* speed. At best, very little on the positive side is known about this "Psychomotor Precision" factor as tentatively identified on the Air Force tests.

Steadiness. This factor has been discussed by Seashore and his coworkers (**28**). The indication is that coordinations emphasizing accuracy (precision or steadiness) while minimizing speed and strength tend to cluster together. Studies bearing on this factor include those of Buxton (**4**), Spaeth and Dunham (**35**), Humphreys, Buxton, and Taylor (**17**), and Seashore, Dudek, and Holtzman (**31**). Tests usually included in this factor are tremor, stylus thrusting at holes, stylus held stationary in holes, and steadiness in tracing in a narrow V slot. The study by Seashore, Dudek, and Holtzman (**31**) on Arm-Hand Precision Tests tested the possibility of finding more than one factor in the range of tests that emphasize steadiness in slower movements or in posture. They isolated three subfactors. These subfactors included involuntary movement of arm and hand, steadiness of movement in a restricted plane (e.g., thrusting a stylus, tracing a straight path towards oneself with a stylus), and steadiness in two- or three-dimensional space (e.g., moving a ring around a rod precisely). However, until a wider range of tests is investigated relative to these subclusters it seems safer to consider a "steadiness cluster" involving precise, slower arm-hand movements and one other grosser kind of precision cluster measured by various postural adjustments such as sitting and standing sway. The moderate relationship between these two clusters has also been shown by H. G. Seashore and Koch (**23**) and Seashore, Buxton, and McCollom (**30**). This latter factor may be like the factor of kinesthesis tentatively identified in the Air Force battery.

Motor Kinesthesis. The existence of this factor still rests upon an insecure basis although it has appeared in several Air Force analyses (**13**, **18**, **19**). The factor was almost entirely confined to the Rudder Control Test. This test consists of a simulated cockpit in which the subject has to make compensatory motor responses to keep the unit balanced in a given position. He does this by pushing rudder control pedals but the displacement of his entire body from the position of equilibrium brings into play kinesthetic and tactual factors. The test also has loadings on the Psychomotor Coordination factor previously discussed, but the two factors may be separate. Later Air Force analyses included pilot interest and pilot experience scores loaded on this factor. How this factor relates to the factor common to

standing and sitting sway identified by Seashore, Buxton, and McCollom (**30**) in their studies remains to be demonstrated. Including tasks of the latter type in batteries with the Rudder Control Test might clear up some of these points.

Aiming (*Paper-and-Pencil Psychomotor Speed with Precision*). This factor represents the ability to carry out quickly *and* precisely a series of movements requiring eye-hand coordination. Unfortunately, all tests yielding this factor have been of the paper-and-pencil type and there is yet no evidence that it extends to other kinds of rapid and precise manipulation. Its relationship to Psychomotor Precision, for example, is unclear. However, the United States Employment Service analyses[6] indicate it is separate from the Dexterity factors, the study by Greene (**12**) confirms its distinction from Tapping, and the analysis by Guilford (reported by Melton [**18**]) reveals it is separate from both Tapping and Dexterity. In Greene's factor analysis, the rapid placing of dots in a series of small circles with either hand yielded high loadings on this factor, but none on Tapping. Similarly, tapping in large circles or tapping in no particular spot yielded no loadings on Aiming. In the U.S.E.S. analyses, this separate factor appeared containing tasks requiring speed in dotting in small circles (again the smaller the circle, the higher the loading), rapidly making three lines in each of a series of squares, and rapid drawing of a line through large *H*'s without touching the *H*'s sides. Chapman (**17**), in his factor analysis of Mechanical Ability Tests, found separate Space, Dexterity, and Aiming factors. Although he called the latter "Motor Controlled Manual Movement," this factor contained the same speeded, precise tracing and dotting tasks. Goodman (**11**), with the same battery and a different factor extraction and rotational system, found this same factor. Guilford (reported by Melton [**18**]), in his dexterity battery analysis, found speed of dotting in circles and speed of marking an IBM answer sheet pattern had highest loadings on a single factor and very low to zero loadings on the other four factors isolated. He named this factor simply "Paper-and-Pencil Motor Skill."

In several Air Force battery analyses (**13**, **18**, **45**) the name of "Psychomotor Speed" has been given to a factor including some of these same types of operations. Highest loadings were on log book accuracy (speed in marking in the indicated *A*, *B*, *C*, *D*, or *E* slots on an answer sheet) and marking accuracy (speed in marking an answer sheet under certain circled letters). The Psychomotor Speed label seems too broad a name since there are a considerable number of unrelated psychomotor factors that emphasize speed. Zachert and Shibe (**42**) also found this factor loaded with similar tasks in the U.S.E.S. battery. They defined the factor as "the ability to carry out a series of movements quickly and precisely," which is very similar to our definition of the Aiming factor.

Ambidexterity. This factor has been identified in only one analysis (Greene [**12**]), but may bear further investigation. The tests having principal loadings on this factor were aiming and tapping tests performed with the left hand. Since 90 per cent of the subjects in Greene's study were right-handed, this factor was thought to be the ability to use the nonpreferred hand. The tests with loadings were dotting in small circles, dotting in large circles, and speed of tapping —all done with the left hand. Addi-

[6] See footnotes 2 and 4.

tional studies such as ones requiring left-handed people to use the right hand and studies manipulating handedness in other tasks might help confirm or reject this factor.

Nonmotor Factors in Primarily Motor Tests

Factor studies (as well as experimental ones) indicate that success in complex motor activities may depend upon nonmotor as well as motor factors. Continuing within the framework of previously isolated factors, a few of these nonmotor factors will be discussed briefly.

Some form of *Spatial Relations* seems involved in many psychomotor skill tests. This factor appears to represent the ability to relate different responses to different stimuli, where either stimuli or responses are arranged in spatial order. Thus far, the evidence limits the identification of this factor to tasks involving visual perception only. In at least six separate analyses (**13**, **18**, **19**) this factor was found to be included in some apparatus tests. The Complex Coordination, Discrimination Reaction Time, and Two-Hand Coordination Tests were most consistently loaded on this factor, along with the paper-and-pencil nonmotor tests of Dial and Table Reading, Coordinate Reading, and Instrument Comprehension. Seashore, Buxton, and McCollom (**30**) found a factor they named "Manipulation of Spatial Relations" which included rotary pursuit, assembly, and form board tasks.

Mechanical Experience is another factor which consistently showed up in the Air Force analyses (**8**, **13**, **18**, **19**) as including a motor task. However, the Two-Hand Coordination Test was the only motor test with significant loadings on this factor. Other tests in the factor were Mechanical Information, Mechanical Principles, Biographical Information, Pilot Technical Vocabulary, and Reading Comprehension.

A factor named *Pilot Interest* was identified in later Air Force studies (**8**). This factor was separate from Mechanical Experience and included the Rudder Control Test, general information, and previous flying experience.

A *Perceptual Speed* factor has been found in complex motor tests in some AF analyses but not in others. This factor includes the ability to make rapid recognitions and comparisons of visual forms. The analyses of Dudek (**8**) and Michael (**19**) and those reported in the Air Force volume (**13**) have indicated Discrimination Reaction Time and/or Complex Coordination loaded on this factor. In both these tasks different light patterns are presented successively and must be discriminated rapidly by the subject before he can make the appropriate motor responses. Guilford (reported by Melton [**18**]) found Discrimination Reaction Time, rapid number cancellation, and an arm-hand coordination task of interchanging pegs grouped with two spatial relations tests in a factor he called "Perceptual."

Factors in the *Intellectual-Verbal* area have not been shown to include motor tasks to any extent. They did not seem to enter very much into performance on any of the complex apparatus tests. Although Guilford (reported by Melton [**18**]) found a dexterity test of moving round pegs forward, number cancellation, and simple conflicting manipulations to group with "following directions" and mechanical comprehension tests, his naming of this factor as "Intellectual-Verbal" should be regarded as very tentative, however.

Perhaps just as notable as the presence of some of these factors is

the *lack* of very many nonmotor factors that have been identified in motor performance. However, there was not much opportunity, with the exception of the Air Force analyses, to identify the nonmotor factors in motor task performance since few batteries included both kinds of tests. Future analyses can be aimed also at this problem. From the point of view of developing "unique" psychomotor tests, the effort should be made to minimize in such tests the nonmotor variance measurable by paper-and-pencil tests.

Factor Summary

The psychomotor factors discussed above have generally appeared in two or more factor studies. Although the status of some of these factors is still in doubt, there is considerable agreement about other factors.

The nature of seven of the ten factors discussed rests on more secure ground than the remaining three.

From the limited number of such studies that have been made thus far, the following motor skill dimensions seem to emerge:

1. The speed with which an individual is able to respond, by means of a prescribed movement, to a stimulus when it appears (Reaction Time).

2. The speed with which an individual is able to oscillate either his fingers or his arm, independent of any eye-hand coordination ability (Tapping).

3. The ability to make skillful, controlled arm or hand manipulations at a rapid rate (Manual Dexterity).

4. The ability to make skillful, controlled manipulations with the fingers at a rapid rate (Finger Dexterity).

5. The precision and steadiness with which one is able to make accurate arm-hand positioning movements which minimize strength and speed (Steadiness).

6. The ability to carry out quickly *and* precisely a series of accurately directed movements requiring eye-hand coordination (Aiming).

7. The ability to make somewhat precise postural or bodily adjustments to kinesthetic cues when the body or body members are displaced from positions of equilibrium (Motor Kinesthesis or Gross Precision).

Somewhat less well-defined dimensions seem to be:

8. The ability to integrate gross or fine movements of moderate scope. This factor was identified whenever more complex apparatus tests were used and seems, at present, to be a general muscular agility factor. (It was named "Psychomotor Coordination.") However, it is quite possible that this factor may break down into several more simple factors.

9. The ability of right-handed subjects to use the nonpreferred hand under speed conditions. This has been identified in only one study and was called "Ambidexterity."

10. A factor called "Psychomotor Precision" is an even less defined factor. It may be the same as Finger or Manual Dexterity, but thus far there has been no way of evaluating this. Tests involving this factor seem actually to require more speed than precision.

Nonmotor factors on which certain psychomotor tests have carried loadings include Spatial Relations, Perceptual Speed, Mechanical Experience, and Pilot Interest.

It may be stressed here that only *factor analysis* studies on dimensions of motor skills have been summarized in this section. There are other ways of categorizing and describing such skills. However, it is felt that factor analysis provides the best available

technique for classifying skills into the smallest number and most independent set of categories that might account for performance on a wide variety of tasks.

Possible Research Approaches

Factor analyses of psychomotor abilities. From the foregoing review it can be seen that much additional research is needed to clarify a dimensional analysis of motor abilities. It is the view of the present author that the major emphasis in future research in this aptitude area should be given to systematic investigations of basic psychomotor ability dimensions. In the preceding section some starting points for more definitive analyses are suggested by the previous factors identified, tentative factors needing confirmation, and the limited range and complexity of tests utilized. It would seem that the construction and factor analysis of comprehensive, specially designed psychomotor test batteries are basic needs in this area. Such research would help to get us closer to the isolation of the relevant variables in motor performance with respect to aptitude test development.

On-the-job surveys. Surveys of the specific motor skills which appear to be involved in various jobs might provide assistance in designing tasks to be included in factor analysis batteries. Various job analysis approaches might be tried, although the analyst would have only traditional motor skill categories available. As a side study, it might be possible to "job analyze" a well-controlled complex motor task along different kinds of dimensions. Simpler tests could then be constructed to measure the components thus derived. Then it might be possible to see which kind of dimensional analysis could best predict over-all performance on the complex task. Such a study might suggest some possible approaches to on-the-job surveys which would depart from traditional techniques.

However, any kind of careful job study might aid materially in the construction of a meaningful factor analysis battery of motor tasks.

Transfer experiments. Laboratory transfer experiments might be a potent source of data on functionally similar or different aspects of motor tasks. A series of such experiments which systematically varied both the stimulating conditions and the responses involved might yield evidence on common elements in a given variety of tasks. Research on transfer has generally been undertaken within the framework of studying factors influencing *learning.* However, it would seem possible to view such experiments from the standpoint of aptitude testing research, especially where transfer from one task situation to another is studied.

Other experimental investigations. Other kinds of experimental investigations involving motor tasks would give leads with respect to the significance of variables in the motor area. Manipulations of procedural, task, and motivational variables with special attention to patterns of interrelationships might throw additional light on basic factors involved. Investigations of the effects of continued practice on the performance of complex motor tasks might, for example, suggest which tasks bring into play different abilities at different stages of task performance. Or again, changes in the patterns of relationship between different tasks when one or both tasks are practiced might yield additional data on specific or common factors. Such data, for example, might help one decide at which stage of learning scores on certain tasks should be correlated with other task scores for a factor

analysis battery. On the other hand, one might want to include tasks whose correlational pattern is most resistive to change due to practice.

Research on qualitative characteristics of motor responses. This approach overlaps and perhaps should precede the on-the-job survey. It includes observation of laboratory tasks as well as actual job situations and also a literature search from a different point of view from that reported here. Here, one would hypothesize qualitatively different movement characteristics over and above those which have been involved in factor analyses or in traditional classifications. Thus distinctions have been made often between ballistic movements and tense movements, gross versus fine movements, accelerating versus decelerating aspects of movement, manipulative versus nonmanipulative movements, compensatory versus noncompensatory, etc. An example of a suggested classification of motor reactions into fairly distinct types has been provided by Brown and Jenkins (**3**). It might be profitable to include in any empirical dimensional analysis different tasks consisting primarily of such assumed qualitatively different responses to see how correlated they really are and just what factorial composition might emerge.

Small-scale factor analyses. The above sources, which would provide dimensional analysis data and suggestions for a more extensive factor analysis battery, overlap considerably. The collection and integration of all available information and the design of suitable tasks for the large scale analyses would take considerable time. Consequently, small-scale factor analyses on various subareas could be carried out. These analyses would also provide preliminary data relevant to the design of the more elaborate studies.

Some Methodological Problems

There are some additional issues in the area of psychomotor testing which overlap somewhat with those previously discussed. Some of these will be presented briefly since they also have important bearing on the directions which psychomotor test development might take.

Job-Sample Tests Versus Tests of Basic Abilities.

Aptitude test development for the selection of personnel for a particular job often takes the form of tests that closely resemble the job both in content and complexity. The assumption here is that the more nearly the test resembles the job or some phase of it, the more accurately test performance will predict job performance. Because the job is usually complex, requiring a number of different operations often at the same time, the tests themselves become complex. This is especially true in recent psychomotor test development and was illustrated in the Air Force wartime apparatus testing program. Thus in developing tests for pilot selection a large number of complex pursuit and coordination tests were developed. Subjects were required to respond to a variety of signals and cues. Even more to resemble the job, the tests often used airplane-type stick and rudder controls and instrument panels. An additional advantage of such psychomotor tests is their high face validity for both the sponsor of the research program and the examinee. This, however, in the case of some subjects may be a disadvantage. The subject may be negatively motivated in tests which he assumes, rightly or wrongly, will qualify him over another preferred job. Such tests did, however, prove to have considerable actual

validity for predicting the particular criterion of success used. It certainly seemed that the validity of these complex tests was greater than could have been achieved by any combination of simpler tests available at the time.

However, as might be expected, each test of this type correlated quite highly with all other complex apparatus tests developed for the same job. Furthermore, as Thorndike (**37**) has indicated, such tests overlap not only in their valid variance, but also in their invalid variance for each job. Moreover, errors in interpreting the job also may be perpetuated in each test. The high intercorrelations indicate that relatively little gain will result from adding additional complex apparatus tests to the battery. However, as Thorndike also points out, whether the final multiple correlation will be higher for a program based on complex job analogy tests with relatively high validities and high intercorrelations or on relatively pure tests of simple functions with lower validities and lower intercorrelations remains an open question.

The issue becomes increasingly important, however, when the problem of *multiple* selection or *classification* rather than simple selection is considered. Although it would be theoretically possible to devise a separate battery for predicting success in each of several jobs to which applicants might be assigned, such an approach would become hopelessly inefficient, unwieldy, and time-consuming with a larger number of job categories. Thus, it becomes necessary to use each test for predicting success in several jobs. In the classification situation, then, it becomes less defensible to design the test in terms of a particular job. The problem is further pointed up by the fact that even the individual job itself may change. For example, the Air Force pilot of 1960 may have to bring into play different combinations of abilities from those of the pilot of 1950.

The alternative to such job-sample tests in the psychomotor area is tests designed more in terms of general psychomotor ability categories. These tests may still be apparatus tests and may be complex mechanically and operationally, but not complex factorially; that is, each test will sample as nearly as possible only one motor "ability" category. This approach to psychomotor test development, then, starts off with the search for basic motor ability categories suggested above.

Are Complex Psychomotor Tasks Qualitatively Different from Tasks of Simpler Motor Functions?

There is a fundamental assumption underlying much of the discussion in this paper—the assumption that it is possible to break down the variance in performance of complex psychomotor tasks into simpler more fundamental psychomotor functions. As was indicated earlier, many previous attempts at predicting performance in complex motor tasks from skill in simpler tasks have failed. However, these studies did not utilize tasks representing empirically derived factors in the complex task. In the few cases where this was done (e.g., in the steadiness and finger and manual dexterity areas), predictions were more successful.

By "simple" tasks, the author does not necessarily mean such tasks as finger oscillation, peg-turning operations, or reaction-time functions, but rather, tasks which are not *factorially complex*. Thus, a task requiring a certain kind of coordination measured by complicated apparatus may turn out to be "simple" in the sense of sampling an ability area that may

vary relatively independently from another motor ability area. The apparatus complexity required for tests measuring many of the yet undefined motor skill factors may lie somewhere between the very elementary operations requiring a minimum of apparatus and the complex job-sample type tests now in use.

However, whether the introduction of complexity of function introduces valid variance not covered by *any number* of more analytical tasks of simpler motor ability functions is a point still in question. From the success of such relatively "pure" tests in other aptitude areas, it must be assumed that it is possible to predict performance efficiently in complex tasks from combinations of tests involving more basic categories of ability. Whether the abilities involved in complex psychomotor tasks involving a number of operations are qualitatively different from the abilities involved in the more analytical tasks remains a subject for future research.

Apparatus Tests Versus Paper-and-Pencil Tests of Motor Abilities

A few studies, previously mentioned, have shown that some paper-and-pencil tests have reproduced some of the variance involved in the performance of certain apparatus tests. Thus, various tests of spatial relations and perceptual speed, and the mechanical experience key of a biographical information blank were moderately correlated with some complex apparatus tests in the Air Force batteries. Tests of these kinds have been regarded as measuring *nonmotor* factors involved in primarily motor tasks.

On the other hand, some paper-and-pencil tests have been designed primarily as motor tests (i.e., tracing, dotting, aiming). That such tests can possibly reproduce the variance associated with more complex apparatus tests remains to be shown. In several studies reported earlier, batteries containing both paper-and-pencil and apparatus motor tests often yielded a factor unique to the paper-and-pencil tests. Whether this is due to the restricted range of tests in the batteries or to some actual differences in motor abilities tapped by paper-and-pencil and apparatus tests needs to be demonstrated. More paper-and-pencil tests specifically designed to sample certain psychomotor functions need to be constructed and correlated with apparatus psychomotor tests. Such research might prove even more significant if the factorial content of the apparatus tests were known.

The ultimate utility of paper-and-pencil motor tests lies in their validity. There is little doubt that if ever any number of such tests can be shown to predict a certain apparatus test score efficiently they should be substituted for it.

Motion-Picture Tests

Nothing has been said thus far concerning the role of motion-picture tests in motor skill aptitude testing. This is because motion-picture tests have been used in the past to test functions primarily perceptual rather than primarily motor in nature.

The use of motion-picture tests in the wartime Air Force program has been summarized in Gibson's volume (**10**). General areas in which these tests were constructed included (*a*) tests of ability to judge motion and locomotion, (*b*) tests of ability to judge distance, (*c*) tests for orientation in space, (*d*) tests of ability to perceive slight movement, (*e*) tests requiring multiple perception, (*f*) tests involving sequential perception, (*g*) tests of perceptual speed, and (*h*) tests of comprehension. In all cases,

the testing emphasis was to measure accuracy of perceptual discriminations or judgments of a psychophysical nature which could not be adequately measured by printed tests. Thus, the developers of these tests were not at the time interested in measuring the *motor aspects* of responses to the stimulating conditions presented. The examinees, in fact, recorded their answers on standard IBM answer sheets.

Considerable thought might be given to the use of motion-picture projections, or series of slide projections, as an integral part of apparatus tests used in the *motor* ability area. The problem of synchronizing and recording differential response pattern with photographic stimulus presentation is a crucial and difficult one, however, and would have to be solved before such tests could be useful. One would have to investigate carefully if photoelectric synchronization could be made to meet the standards of precision demanded for proper calibration and standardization of such tests.

With the possible exception of the Pedestal Sight Manipulation Test (**18**), the use of photographic means of presentation has not been very much explored in the motor skill testing area. Such presentation might allow, for example, a more economical means of testing for motor skills with larger numbers of subjects at a time. Moreover, such stimulus presentation might be more flexible than one involving more complex apparatus and lighting systems. Thus, one could easily vary the speed of presentation, exposure time, and the intervals between presentations. Or, again, one could keep the mode of response constant and change the stimulating conditions by merely substituting a reel of film or a different set of slides. Similarly, one could provide for different kinds of responses to the same stimulus patterns. Such an adaptation of motion-picture tests might thus aid materially in the search for the important dimensions of motor skills by experimental methods. For example, a need in this regard is to hold the perceptual elements of a given range of motor tasks constant while varying the response dimensions. Many of the previous correlational analyses of motor task performances have employed tasks in which both the stimulating conditions and the responses required were allowed to vary.

Work Methods

The importance of work methods in success in fine motor skills has been stressed by Seashore (**26**, **28**). In fact, it appears to be Seashore's belief that "hitting upon" favorable work methods may account for a greater proportion of individual differences in certain groups of operations than any combination of basic motor abilities. This hypothesis seems to raise a question about how well we can ever expect to predict success in certain motor tasks from any standard testing procedures. Although the work-methods hypothesis is important in relation to training in fine motor skills, it might be well to consider it from the point of view of selection.

One might test the hypothesis that for a given range of tasks, a person brings with him a general way of attacking such problems and that differential "work habits" may be predictive of success in complex psychomotor or manipulative tasks. The crucial problem here initially is that of describing and measuring such work methods. This is as yet

an unsolved problem. One might merely wish to consider efficient work methods identical with efficiency of learning. Thus, a standard motor task might be presented to a subject and the speed with which the task is learned to some criterion might be considered predictive of future complex motor learning. Or, again, one might merely wish to observe when the sudden progress associated with "insight" learning occurs during the task. Presumably this would be the stage when useless movements are dropped out and the most efficient work method is applied.

Another approach might utilize participant-observer techniques or introspective reports of individuals performing standard complex motor tasks. A classification of habitual approaches to a range of difficult psychomotor tasks might be organized and a weighted checklist constructed which could then be validated against job success.

Another approach might make use of automatic recording devices such as "operation recorders" which trace response patterns on a tape moving at a given rate of speed. Such a recorder hooked up in the proper fashion to a complex coordination test might indicate (*a*) what sequence of movements the subject used, (*b*) when the subject attempted to move simultaneously in more than one plane at a time, (*c*) when wrong directional moves were made, (*d*) when the subject was moving too far in a given direction, and (*e*) when the movements became smooth and coordinated.

Records indicating the subjects' work methods during performance of a well-controlled psychomotor task would be even more meaningful when the work methods of subjects who score high are compared with those of subjects who score low on total task performance.

Selection Versus Training with Respect to Motor Skills

Related to Seashore's "work-methods" hypothesis is the possibility that muscular coordination, manipulative dexterity, etc. are best considered as training problems rather than as selection problems. There is some feeling that, although people differ in their motor skills, the kinds of motor skills involved in certain trades and machine operations are highly amenable to training. After training, people may become relatively homogeneous with respect to these skills. Thus Tiffin (**39**) has indicated that the tradesman usually succeeds or fails in proportion to his training and general mechanical comprehension, not in proportion to his basic dexterity. This does not mean that successful tradesmen do not need skilled movements but rather that such muscular coordination as may be needed can be developed in training and it is a lack of some other ability (e.g., mechanical comprehension), rather than inability to develop the muscular aspects of the job, that may prevent proficiencies in the job. Similarly, Harrell (**16**) feels that success in such jobs as textile-working involves more the ability to visualize certain aspects of the job and only to a negligible extent muscular dexterity. There have been too few studies on final level of motor skill attainment to evaluate these hypotheses properly. Even in the Air Force program, where psychomotor tests proved to have considerable validity in predicting training criteria for pilots, navigators, and bombardiers, little study was undertaken to relate the tests to more ultimate criteria in advanced training or in

operational settings. Thus, it may still be that differences in these skills contribute less to the variance in job performance as training is continued.

Even though it may be possible to reduce individual differences appreciably through training, there is still the question of individual differences with respect to training time required. From a cost point of view, this is still a highly desirable criterion to predict by means of psychomotor tests. Moreover, individuals requiring more intensive training on motor skills in order to complete training in a specified time might be identified by such tests. Research is needed to investigate the relative importance of a training versus selection approach in a given range of motor skills. Experimental laboratory studies investigating individual-difference variables in final and intermediate levels of skill attainment as well as studies predicting job and training success would seem relevant research areas basic to the future of psychomotor test development.

Summary

In this paper, an attempt has been made to examine the area of psychomotor skills research from the point of view of aptitude test development. The historical background of psychomotor test development is presented, problems attending the use of such tests are discussed, and previous factor analysis studies in this aptitude area are summarized. Suggested directions of future research are discussed in terms of possible research approaches and certain methodological problems having research implications.

While test development in other aptitude areas has been increasingly aimed at tests sampling one ability category at a time, tests of psychomotor skills have become more complex in the number of abilities sampled at one time and in the types of testing apparatus used. It appears that a basic need in this area is an extensive dimensional analysis of motor abilities which would reveal a more basic classification of factors primarily important in the performance of a wide variety of psychomotor tasks. Research of this nature has been fragmentary and for the most part has investigated only a limited range of psychomotor tasks. However, the review of previous factor analyses of psychomotor tests does reveal surprising agreement between studies on previous factors isolated. The previous factors identified, those needing confirming, and gaps in the limited range and complexity of tests utilized offer some starting points for a more extensive dimensional analysis of motor skills.

In addition, further sources of hypotheses about motor skill dimensions are suggested. These include on-the-job surveys, transfer studies and other kinds of experimental investigations of motor skill variables, research on qualitative characteristics of motor responses, and small-scale factor analysis studies.

With respect to methodological issues, the value of complex "job-analogy" type apparatus tests decreases when we are faced with a classification rather than a selection problem. The problem of classification intensifies the need to find the most independent set of motor ability categories, tests of which could be weighted differentially in predicting efficiently for a wide variety of jobs. Also questioned is the assumption that the abilities involved in performance of complex psychomotor tasks are qualitatively different from the abilities involved in factorially more

simple tasks. Research on this problem systematically relating performance on complex tasks to performance on a range of "pure" tasks is needed.

The possibilities of paper-and-pencil tests and the role of motion-picture presentation in the psychomotor area are also discussed.

Possible research on the quantification and measurement of "work-methods" is pointed out. The implication here is that for a given range of tasks individuals bring with them a general way of attacking such problems, and that differential "work habits" may be predictive of success in complex psychomotor or manipulative tasks. Such measurements of "work habits" might thus provide additional predictions of later performance.

Studies relative to the problem of selection versus training with respect to motor skills are suggested. Experimental studies involving individual differences on final and intermediate levels of skill attainment and studies predicting job and training success at different stages are examples of relevant research needed on this problem.

It is hoped that this paper will help provide a framework within which some future research on psychomotor skills testing can be carried out. Several lines of possible research have been discussed, the primary one being concerned with a dimensional analysis of motor skills as a basis for future test development. It is apparent that much additional research in this aptitude area needs to be done before the utility of such testing can be properly assessed.

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THE USE OF THE FREE OPERANT IN THE ANALYSIS OF BEHAVIOR[1,2]

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The increasing number of experiments in the literature using the Skinner box makes it more important that the methods and techniques of this kind of experimentation be public. Since the technical problems of the experiments do not bear centrally on the analysis and would prove burdensome to the general reader, it is understandable that published accounts of this type of research do not include detailed specifications of all the techniques employed. Yet it is becoming impossible to repeat experiments solely on the basis of the published accounts. This paper will describe some of the criteria and principles which would make it possible for the interested researcher to duplicate the conditions of free operant experiments.

The Skinner box is generally considered a special technique employing an apparatus design of the kind originally used by Skinner (**1**). Any piece of apparatus differing in any way from the original design has been termed a modified Skinner box. Contrary to this view, the Skinner box is not a specific technique, but rather a method of research employing the free operant. The use of the free operant is a method of wide generality; it refers to any apparatus that generates a response which takes a short time to occur and leaves the animal in the same place ready to respond again. The free operant is used in experiments when the main dependent variable is the frequency of occurrence of behavior. Nearly all the problems of a science of behavior fit this paradigm when the questions are of the form: what is the likelihood of a piece of behavior occurring on this particular occasion; how strong is the tendency to behave on this occasion relative to another occasion? The free operant has advantages in this respect, because it removes restrictions on the frequency with which a response can occur and permits the observation of moment-to-moment changes in frequency (**4, 5**). The behavior of approaching the food magazine and eating, which is insensitive to a large number of variables, provides one such restriction. When the free operant is placed on an intermittent schedule of reinforcement, the small number of reinforcements relative to the number of responses minimizes the restriction on frequency. With proper techniques, animals can be trained so that the behavior of approaching the magazine and eating is under good stimulus control and, for all practical purposes, an invariant.

[1] This work was carried out under Contract N5ori-07631 between Harvard University and the Office of Naval Research, U. S. Navy (Project NR143-943, Report PP-1). Directed by B. F. Skinner.

[2] A large number of persons have contributed to the techniques summarized in this paper so that it is impossible to establish credit for my particular idea. Credit for his part in the design of much of the apparatus mentioned in this paper is due to Mr. Ralph Gerbrands, 96 Ronald Road, Arlington, Mass.

[3] The author has been collaborating with Professor Skinner since 1950 on a program of research (sponsored by the Office of Naval Research) on the intermittent reinforcement of operant behavior.

Since the form of the free operant is arbitrary, a further advantage can be gained by choosing a response which has a zero or near zero unconditioned rate. Frequency of response under an intermittent schedule of reinforcement generates a dependent variable which can take values over a very wide range, and which can serve as a base line for judging the effect of many variables. The range of the dependent variable is large. The frequency of a pigeon's pecking response, for example, can vary from zero or a few responses per hour to 20,000 responses per hour; over shorter periods rates have been observed as high as 15 responses per second.

Instrumentation of the Free Operant Situation

Instrumentation of the free operant situation requires the following: a manipulandum, i.e., some device which the animal can manipulate and which operates a switch; a recording system; a magazine to present and control the reinforcing stimulus; a small chamber into which the animal and apparatus can be placed; and control equipment to arrange the required contingencies between the operation of the manipulandum switch, magazine, recorder, and any stimuli that might be presented. These components will be taken up one at a time. Since the principles of instrumentation will be clearer from examples, apparatus and procedures for the pigeon will be described. The methods are general, however, and the applications to other organisms may be induced from the pigeon techniques. In addition, an extensive series of research reports covering work on the pigeon will be published shortly which will illustrate the methods and techniques of this paper.

Manipulandum

The problem in the design of a manipulandum is one of generating responses by reinforcement that will be of similar form so that all of the members of the class will be recorded and covary with the manipulations of the independent variables. As an example of the kind of difficulty that can be encountered in this respect, consider the case of a pigeon key which requires a forceful peck to actuate the switch. Since the magazine is operated only when the peck is of sufficient force to operate the key switch, the force of the peck will be differentiated. Some variation will occur, however, with some pecks stronger than necessary and some pecks not having the required force to operate the switch. It must be decided whether the pecks which do not actuate the switch, and therefore go unrecorded, are to be counted as pecks. The problem is one of the generic nature of the response class and can be settled only by examining the lawfulness of the behavior that has been recorded. The difficulty can be avoided by designing the key so that the lightest peck that is expected will operate the switch.

Since automatic programming and objective recording of the experiment require that the response being measured operate some switch, other problems in the definition of a response can occur. It is possible, for example, to design a manipulandum that can be operated by behaviors which have extremely different topographies. In such a case it would not be clear whether unexpected observations were artifacts from variations in topography or attributable to their experimental manipulations.

The selection of the pecking response in the pigeon is a particularly fortunate one since it is already in

the repertory of the pigeon and occurs with little variation in the topography other than the force of the peck. The pecking response would, of course, be extremely unsuitable where variables influencing the form of the behavior were being manipulated.

The occurrence outside of the experiment of the behavior which is measured is another factor which must be controlled by training procedures and apparatus design. This is done by selecting a response which is infrequent in the natural repertory of the animal, by making the manipulandum prominent, or by placing the behavior under complete stimulus control by proper training procedure. The pecking response of the pigeon does not offer difficulty in this respect since the unconditioned level of pecking on a wall surface is almost zero; after the pigeon has been taught to peck, the light behind the key comes to control the behavior to the extent that if the color is changed the pecking stops.

The design of a key for a pigeon offers special problems because of the extremely high rate of response that can be generated. Rates of response as high as 15 per second can occur under appropriate schedules of reinforcement. The advantages that accrue from the wide range of values over which the rate of pecking varies are lost if the key does not have a sufficiently high natural frequency. A high natural frequency is obtained by making the armature of the key extremely light and the distance the armature must travel in order to close the actuating contact as small as possible.

Several types of keys have been in use in the Harvard laboratory, none of which are satisfactory in every respect. A key that is used at present employs a bakelite frame on which is mounted, on bearing surfaces, a piece of clouded Plexiglas. Relay contacts are mounted on the bakelite frame on phosphor bronze with the opposing contact on the Plexiglas. When the key is operated, the flexing of the phospher bronze supplies a slight wiping action which cleans the contacts. A spring holds the Plexiglas armature against the bakelite frame and the resulting contact arrangement is normally closed. The maximum frequency of operation of the key can be increased by increasing the tension of the spring. Conversely, however, pecks will have to be executed with greater force in order to be recorded. The usual solution is a compromise between maximum frequency of operation and minimum force necessary to actuate the switch. Where it is possible to predict the rates of response that will be encountered, one practice is the use of a heavy spring on the key armature where uniformly high rates of response are expected, and a reduced spring tension where uniformly low notes are expected. Because of the millions of times the key will operate, the key contacts must be protected from heavy current loads and sparking. This is done by isolating the key from the rest of the programming circuit by a spark-suppressed relay of high enough impedance. Since the required duration of a peck before it can be recorded depends on the operate time of the keying relay, the relay chosen must be such that a minimum time elapses between the application of a voltage and the operation of the relay. The key switch is normally closed, because the time between the application of a voltage to the relay and the closure of its contact may be as much as 40 milliseconds longer than the time

between the discontinuation of the voltage and the closure of the normally closed contact. Since the speed of operation of a relay tends to be inversely related to its impedance, a compromise is made by choosing a relay whose impedance is low enough to ensure fast operation yet high enough that excessive current loads are kept from the key contacts.

A key which is adequate for many applications can be constructed by hinging a piece of Plexiglas at the top and placing a limit switch at the bottom, behind the Plexiglas. The movement of the Plexiglas either makes or breaks the limit switch contact. The characteristics of the key can be adjusted by changing the thickness of the spring or the distance between the contacts on the spring.

The optimal size of key has not been explored, but a key 1 inch in diameter has been found to be satisfactory. The pigeon reaches the key through a 1-inch aperture cut in the wall. The height of the key from the floor depends on the size of the bird. For the homing pigeon this distance would be $7\frac{1}{2}$ inches from the center of the key.

Recording

The most straightforward recording arrangement that could be used would be a polygraph. This would give a complete record from which any kind of computation or presentation could be made. The amount of work necessary to transcribe the polygraph record into a form that is usable makes this kind of recording unfeasible, however. This would be true for any experiment employing a free responding situation, but particularly true for experiments with pigeons, where as high as 20,000 responses an hour can be recorded. Polygraph records also result in a loss in efficiency because the experimenter can have little notion of the state of the experiment until a summary is made by laborious transcription. An even more serious difficulty is that the use of a polygraph precludes manipulations of variables in the middle of experimental periods.

It is the practice in the Harvard laboratory to devise recording instruments for each experiment which summarize the data in that aspect of the dependent variable which will be used in the analysis. The kind of recordings that are taken usually arises after considerable exploratory work. The record most frequently taken is cumulative. Other types have been used, however, and the following experiment is an example of use of a summary record of a noncumulative type.

The latency of a response to a discriminative stimulus was being studied. After preliminary exploration, the experiment was programmed so that the light behind the key came on once a minute. The response to the light was reinforced and the light was turned off. The time between the onset of the discriminative stimulus (the light behind the key) and the occurrence of the reinforced response was measured and an arbitrary value within this range was selected. This was called the criterion time. If the time between the onset of the light and the occurrence of the response was greater than this value, the response was not reinforced and the criterion time was lengthened by a fixed amount. If the response occurred within the criterion time, the response was reinforced and the criterion time was shortened. The data were analyzed by registering the changes in criterion time. The paper

was driven at a constant speed by a clock motor; the pen, which travels perpendicular to the direction of the paper drive, moved a constant distance up the paper whenever the criterion time was increased and moved a constant distance down the paper whenever the criterion time was decreased. The resulting record could be examined at a glance and showed concisely the change in latency over the experimental period.

Whenever an intermittent schedule of reinforcement is used, a cumulative record generally proves to be the most convenient and useful method of recording. The cumulative record represents nonprocessed data, in the sense that it is drawn by the bird directly. If a proper scale is chosen, it is possible to recover all of the information that would be at hand if a polygraph record were taken. In most cases, however, this choice of scale would vitiate the advantages of the record. The cumulative record is drawn as follows: A pen is stepped across the paper a small distance for each response. At the same time the paper feeds at a constant speed. The slope of the line that is drawn is directly proportional to the rate of the response. The virtue of the cumulative record is not that it allows a precise measurement of the rate at any particular time, but rather that it emphasizes changes in rate which can be seen in the curvatures of the record. Continuous rate changes occurring over a period as long as several hours can be summarized in the raw data of a cumulative record. In addition, subtle variations such as the "grain" of the rate show up on local curvatures.

The choice of a scale is dependent upon the range of rates that are expected. These in turn are dependent upon the species of animal used, the operant chosen, and the schedule of reinforcement. While the rate of response is directly proportional to the tangent of the slope over the whole range, it is the angle that the tangent makes with the abscissa that is actually observed in inspecting the local variations in the record. The rate of change of the rate of response with respect to the angle of the tangent to the curve increases very rapidly as the angle becomes large. At values higher than 80 degrees the tangent of the angle increases so rapidly that changes in rate over short periods of time (the fine grain of the record) are impossible to measure. The scale that is selected is therefore such that the highest rate expected will produce a line whose angle is no more than 80 degrees with the abscissa.

A convenient scale for use with pigeons on interval schedules of reinforcement would be a paper feed of approximately 11 or 12 inches per hour and 1,000 responses for a 6-inch excursion of the pen. For use with rats, this scale would be much too reduced and a better choice would be either a slower paper feed or fewer responses per excursion of the paper. Where both fine grain effects and over-all trend are desired, it is often the practice to use two recorders, one of which gives a much reduced record.

In order not to contaminate the record with eating time, the programming equipment is arranged so that the paper drive stops during the operation of the magazine. A marker is used on the recorder to indicate the response which was reinforced.

In discrimination experiments it is the practice to use tandem recorders, one for S^D and one for S^Δ. Both the S^D and the S^Δ records are internally orderly and show a degree of lawfulness which would not be immediately

apparent if the behavior under the two stimuli had not been recorded separately. In experiments on concept formation four recorders have been used in tandem. The limiting factor in the most useful recording arrangement, in this type of experiment, is the expense of the recorder.

Magazine

The magazine is the device by which the reinforcement of the free operant is instrumented. In the design of the magazine it must be recognized that in conditioning a response a chain of responses is being formed, and that the critical event is the one immediately following the response. When we wish to reinforce a peck we must follow it by a prominent event which is the discriminative stimulus for the first member of a chain of responses leading finally to the ingestion of food. The concern with the chain of events, which maintains the reinforcing properties of the stimulus following the peck, is simply that it occur with regularity and uniformity under good stimulus control.

The types of magazine that can be used have two kinds of effects, a fixed amount of food or a fixed period of access to food. While the first of these guarantees that the same amount of food will be eaten for each reinforcement, it makes difficult the maintenance of good stimulus control over behavior in respect to the magazine. The amount of time the animal spends at the magazine will vary because there is no clear-cut stimulus correlated with the end of the food delivery. In addition, it is impossible to know whether the correct amount of time has been subtracted from the records. A more serious difficulty is encountered when the bird leaves food in the magazine and finds it later, in the absence of the discriminative stimulus for approaching the magazine. The partial abolishment of the magazine discrimination results in behavior which will be in competition with the pecking response.

The magazine which presents food for a fixed period of time eliminates all the difficulties that are encountered in the fixed-amount presentation magazine, but suffers a disadvantage in that the amount of food ingested for each reinforcement is not necessarily uniform. Nevertheless, the fixed-time presentation magazine is undoubtedly superior for most purposes, because the variability introduced by slight variations in the amount of food per reinforcement is small in respect to the difficulties encountered from competing behavior conditioned in the same terms as the dependent variable of the experiment.

The natural diet for the pigeon is almost entirely grain, and grain turns out to be a suitable reinforcing stimulus, both from the point of view of the bird's behavior and the construction of a reliable magazine. The grain used in the Harvard laboratory consists of a mixture of 50 per cent kaffir corn, 40 per cent vetch, and 10 per cent hemp seed. The grains tend to be of uniform size.

The magazine now at the Harvard laboratory works as follows: A solenoid draws a pivoted tray into a horizontal position where the pigeon can reach the grain through a small aperture; a funnel feeds grain as needed at the rear of the tray; a 6-watt lamp, mounted directly over the aperture through which the pigeon eats, lights whenever the tray is raised to the eating position; when the tray is dropped out of reach the light goes out and the resulting il-

lumination is sufficiently dim so that the pigeon cannot see the grain. The illuminated magazine serves as an S^D for approach to the magazine and its termination serves as an S^Δ in respect to pecking in the magazine. After only minutes of training the bird makes no attempt to reach the grain when the tray is in the removed position and the magazine light is off.

Other types of magazines which move the grain in and out of the bird's compartment, or which cover or uncover the grain, have been used but suffer the disadvantage that the bird can be hurt by having its bill caught in the moving part of the magazine. While this will occur only rarely before the bird learns to avoid being caught by the moving part, the resulting timidity in respect to the magazine will make the magazine training much more difficult and introduce a variable into the experiment which can be easily eliminated by the design of the instrument.

The Experimental Chamber

The size of the chamber in which the bird works is to a large extent arbitrary, so long as the magazine is positioned in the neighborhood of the manipulandum. Limitations in space, however, usually require that the apparatus be kept as small in size as possible. A suitable size for a bird would be approximately $15 \times 12 \times 12$ inches. When the bird compartment is this size, artificial ventilation is almost always required.

Sound insulation is a knotty problem whose solution depends upon the taste of the experimenter and the requirements of the particular problem. The physics of sound absorption inevitably requires insulation by materials of considerable weight and thickness. This solution is both very expensive and space-consuming. For most applications the degree of sound proofing that is afforded by a well-constructed picnic icebox will provide data which are unaffected by random noises that occur in the experimental room. Critical events which can form the basis of a discrimination, however, must be silent. Since these events are nearly always the operation or the release of a relay, the problem can be solved most easily by using high impedance, light-weight relays that are shock mounted. The low intensity of these clicks, together with the amount of sound insulation applied by a good quality picnic icebox, will make impossible the formation of the discrimination based on a relay operation. In addition to these precautions it is the practice of the Harvard laboratory to supply in each experimental chamber a masking noise consisting of a random spectrum of sound. This type of noise is most effective in masking clicks. For nearly all applications, there is no need to eliminate the recorder noises. The click which the recorder makes every time a response is made serves as a conditioned reinforcer for the response and helps maintain a stable form of the response.

Control Equipment

Because the response that is chosen in the free operant situation actuates a switch every time it occurs, it is nearly always possible to instrument an experiment so that it runs automatically. Once the programming equipment for an experiment has been devised it requires very little attendance by a psychologist, and essentially unskilled personnel can run the experiment. Apparatus does double duty, since it can run all night without any attendance. Night experiments are often an efficient

solution where it is decided to minimize outside sound disturbances. The limiting factor in the number of experiments that can be carried out under conditions of automatic programming is the ingenuity of the psychologist, the amount of money available for programming devices, and competent technical assistants.

Night programming of experiments requires some method of terminating the experiment in the absence of the experimenter. The problem is easily handled in the case of the pigeon because the pigeon roosts in the dark. To terminate the experiment, it is necessary only to disconnect the reinforcing circuit and turn out all of the lights in the apparatus. When a nocturnal animal is used, however, external events are correlated with the termination of the experiment and a discrimination is formed.

There are no simple rules or procedures for the automatic programming of experiments. The program is accomplished almost exclusively by the relay and timing devices, and there is no substitute for general relay know-how or technical assistance. Facility for designing relay circuits for use in programming experiments comes with a small amount of practice.[4]

Care must be taken in the design of relay circuits so that the length of pulse necessary for the operation of the relays in the recording circuit is not larger than the length of pulse necessary for the operation of the relays in the reinforcing circuit. If the operate time of the reinforcing circuit is shorter than the recording circuit, the force or duration of the pecks will be differentiated so that large numbers of pecks will occur which are of sufficient force to operate the magazine, but not capable of operating the recorder. One solution of this problem is to require a larger duration and intensity of pecks for the reinforcing circuit than is required for the recording circuit. In this way the mean force or duration of pecks that occur is increased, and the likelihood of nonrecorded pecks is correspondingly decreased.

[4] An excellent text on general principles of relay circuit design is available in *The Design of Switching Circuits*, by W. Keister, A. E. Ritchie, and S. H. Washburn, D. Van Nostrand, 1951.

The Pigeon as an Experimental Animal

The pigeon has advantages over the rat as an experimental animal that justify the use of a new animal in spite of a considerable body of literature already dealing with the rat.

The pigeon lives as long as 15 years. This makes possible long-term experiments, into which the maturation of the rat would introduce radical changes. A variable interval schedule, for example, which took 30 or 40 days to reach a steady state, would result in an experiment which would encompass an important fraction of the life span of the rat. Considerable economy is achieved by using pigeons sequentially in experiments. For example, if an experiment on some aspects of schedules of reinforcement resulted finally in stable behavior which can serve as a base line, the birds could be reused in an entirely different experiment which required a base line of that sort.

The pigeon has very good visual acuity and color vision. This leaves great latitude and variety in the kinds of stimuli that can be used in discrimination experiments, and the number of different stimuli that can be used within a single experiment. In contrast, the rat's very poor

vision offers serious difficulty in the design of experiments analyzing complex processes.

The pigeon comes to the experimenter with a well-tailored response extremely suitable for free operant type experimentation. The high rates of pecking that can be generated in the pigeon result in a dependent variable which can change over a very wide range, and which is necessarily more sensitive to manipulation.

The Preparation of Birds for an Experiment

The birds should be banded as soon as they are received from the supplier and their wings clipped along the second run of feathers so that they cannot fly. They are fed freely for three or four days, after which time the ad lib. weight is taken. This is designated as the normal weight. After this the birds are not fed again until they reach approximately 80 per cent of their ad lib. weight. This will take about one week if no food is given. An alternative practice, however, is to feed 5 grams of grain every other day. While this will make the deprivation period longer, the disruption of the bird's digestive system will not be so severe. The degree to which any particular animal's weight must be reduced can be ascertained only by attending to behavior. A given percentage of reduction in body weight will not have the same behavioral effect on each animal. Therefore, the weight should be reduced until the animal will eat out of the magazine without hesitation. If the bird does not eat out of the open magazine within 15 minutes after being placed in the apparatus, it should be returned to the loft and not fed for another day.

After the bird eats freely and has had its daily ration on two separate days (15 grams each day, for the typical bird), the magazine training proper can be started. The task at hand at this point is to establish the magazine light and sound as the occasion on which approach to the magazine will occur with high probability, and to extinguish responses to the magazine in the absence of the magazine light and sound. To accomplish this the magazine is operated approximately every 30 seconds. The bird will approach the magazine in the absence of the light, and this behavior will soon extinguish. At this stage of its training the bird should be watched carefully to avoid the development of any superstitious behavior (**3**). If any superstitious behavior develops, it can be eliminated simply by withholding the magazine operation until some other behavior is in evidence. When the bird moves rapidly toward the magazine as soon as it is operated, seldom approaches it when it is not operated, and eats continuously throughout the operation, the magazine training is complete and the pecking response can be differentiated.

The pecking response can be differentiated by any one of three methods. In all three methods the key is connected so that its operation presents the magazine via a switching circuit. In this way, when the first peck occurs the discriminative stimulus for approaching the magazine will be immediately contingent on the response.

1. The first method depends on the unconditioned level of responding. The bird is simply left in the apparatus with the key connected to the magazine. The first key peck that occurs will almost result in con-

ditioning if the magazine training has been effective.

2. The second method uses a discrimination already in the bird's repertory. A small grain is scotch-taped to the key. The bird pecks at the grain and incidentally operates the magazine. Where removal of the grain disturbs the bird, it can be "vanished" by replacing it with progressively smaller grains.

3. The third method is the differentiation of the response. This is done by observing the bird and selecting some behavior which approximates the peck. Often the first step is opening the magazine when the bird is facing the key. The procedure from this point is one of alternate conditioning and extinction of various responses, which progressively approximate key pecking. As soon as the bird is facing the key regularly, reinforcements are withheld until some variation occurs in the behavior which is in the direction of the required response. If reinforcements are withheld too long and the bird extinguishes, the process is begun again.

It is very important that the operation of the magazine be immediately contingent on the specified behavior. If the reaction time of the experimenter is too long, the magazine operation may strengthen other behavior than was intended.

With a small amount of practice and a hungry, well magazine-trained bird, it should take no more than five minutes to "shape up" a pecking response. If there is difficulty in shaping up the pecking response, the bird is probably not hungry enough. The hunger of the bird cannot be judged from the fact that the bird eats readily out of the open magazine, since this behavior is relatively insensitive to changes in the level of deprivation. The frequency of occurrence of the kind of response that is being conditioned is a much more sensitive indicant, and the percentage of body weight reduction that is finally assigned to the bird is best determined by looking at the general amount of activity. If the bird is inactive, conditioning will be difficult and the bird should either be reduced in weight or habituated to the apparatus. A suitable level of deprivation is more easily determined after an intermittent schedule of reinforcement is established. The subjects can be equated in terms of level of deprivation by adjusting their body weight so that the rates of response under a variable interval schedule of reinforcement are equal.

The initial conditioning may be carried out either in the cage that will be used in the experiment or in a specially prepared conditioning apparatus which affords a good view of the bird. Which one is used depends on the conditioning procedure and the type of experimental cage. No particular advantage is gained by the use of an open cage if the conditioning is carried out by the "unconditioned rate" or "grain of corn" method. If the differentiation procedure is used, however, an open cage is necessary, unless the experimental cage has a window which will allow a clear view of the bird when the cage is at a level as high as or higher than the experimenter. The birds will be seriously disturbed if the experimenter looks down on them.

If the pecking response is conditioned by differentiation, some difficulty may result from the accidental conditioning of unnatural pecking topographies or superstitious behavior. This is not likely to be the case where the conditioning is accomplished by the use of a kernel of

grain or the unconditioned rate of pecking. Where an unusual topography does occur, several sessions of continuous reinforcement in the experimental cage will almost always result in a uniform, natural topography.

The length of an experimental session is limited only by the number of reinforcements that are given. The total amount of food that is given is such that the bird's weight will be the same the next day. If the bird does not receive enough food during the experiment to bring its weight up to the value determined as the normal weight, the difference is fed at the end of the session. If the bird gains weight, the number of reinforcements given each session must be reduced. About 60 reinforcements will usually maintain the weight of a bird if a 3-second presentation of grain defines a reinforcement.

The Design of Experiments

There are no rules for the design of experiments on the free operant other than those of accepted scientific practice. Intermittent reinforcement of the free operant, however, allows many experiments to be designed around base lines generated by a schedule of reinforcement. Two examples will clarify how the intermittent reinforcement of a response supplies a base line for judging the alteration of behavior due to an experimental manipulation.

Amount of Reinforcement—the effect of variations in the size of a reinforcement on the tendency to behave.

The pigeon is trained on a variable-interval schedule of reinforcement which generates a constant rate of response. An arithmetic-interval schedule of reinforcement produces a constant rate of response after approximately 15 hours of training. In the middle of an experimental period the amount of reinforcement is either decreased or increased. In this way the effect of the independent variable on the behavior is uncontaminated by any passage of time or handling of the animal. The experiment is then continued until the behavior is stable under the new procedure, and the reversibility of the process is checked by returning to the original amount of reinforcement. If it is found that the process is not reversible, the procedure of alternating the amount of reinforcement is continued in some counterbalanced order. This allows an analysis of the dynamic effect of changes in the amount of food in each reinforcement. If the behavior under different amounts of reinforcement is reversible, the procedure is repeated a sufficient number of times to establish its significance. The number of reversals that are needed depends upon the size of the effect and the lawfulness of the change in the behavior from one amount of reinforcement to another. The parameter is explored over the complete range in a single animal; and the experiment is repeated upon a sufficient number of animals to convince the experimenter that the effects of the manipulation are large in respect to the variation due to individual differences. Relatively few animals are necessary if the records show that the dependent variable changes in the same way from subject to subject, and differs only in terms of a constant, such as over-all level of responding. Sufficient control is possible for use of groups as small as two animals, although a group of three is more efficient statistically. Further refinement of the experiment is necessary if the subjects differ in the dependent

variable in both the constant effect and the nature of the change.

Discrimination

The development of a discrimination between two colors will serve as another example of the use of an intermittent schedule of reinforcement as a base line. After the pecking response is differentiated, the bird is given several sessions of continuous reinforcement and the color of the light changes after each reinforcement. Reinforcement then occurs equally in the presence of the two colors on a variable-interval schedule of reinforcement, and it is not predictable whether the color will remain the same or change after each reinforcement. Separate recorders operating in tandem record the behavior under the two colors. When the rate of response under the two colors is the same, reinforcements are discontinued in the presence of one of the colors. The intervals of the presentation of the color, however, remain defined by the previous reinforcement schedule. As the rate of response in the presence of the nonreinforced color (S^{Δ}) declines, the rate under the reinforced color (S^{D}) does not change. It provides a base line for judging the effects of variables such as level of deprivation.

Despite the alternation of the reinforced and nonreinforced stimuli, the decline in rate in S^{Δ} is extremely orderly. Moreover, after two or three hours of training the rate under the nonreinforced color will be less than 1/100 the rate under the reinforced color. Such an analysis of discrimination problems permits the experimenter to determine over very short intervals of time the tendencies to behave in the presence of both stimuli. The use of an intermittent schedule of reinforcement as a base line for the analysis of the development of stimulus control is easily extended to complex processes, such as concept formation, with similar advantages.

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EXPERIMENTAL STUDIES OF SMALL GROUPS[1]

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In the past few years the study of small groups as an area of experimentation has been attracting the interest of an increasing number of social scientists. A body of literature has been accumulating, upon which it is the purpose of this paper to report.[2]

The field of small-group analysis is here defined as the study of persons in face-to-face relationships. A group must be small enough to provide the possibility of intragroup communication, so that each member, after a group meeting, may report impressions and thoughts about every other member if so required. Many of the studies involve the direct observation of artificially formed groups in the process of solving problems presented by an experimenter under controlled laboratory conditions. It makes no difference for definitional purposes whether members are acquainted prior to the meeting of the group, but naturally this factor is of prime concern in experimental design.

Priority for inclusion in this review of experimental studies has been given to controlled laboratory investigations rather than descriptive field reports of groups in which there has been little attempt to define precisely the variables, both dependent and independent, or to seek relationships between two or more observed behavioral indices. Since the major purpose of this review is to evaluate the present state of empirical knowledge in the field of small group study, those articles in which emphasis is primarily theoretical or interpretive have been eliminated. Thus, the work of early thinkers such as Cooley, Elliot, Follett, LeBon, Mead, Simmel, Sumner, etc., who are important for their many shrewd insights and hypotheses about groups, is not dealt with directly but only indirectly in so far as it has stimulated and proven to be amenable to experimental investigation. Similarly, recent systematic treatments of small group phenomena such as those given by Bales, Bion, Homans, Moreno, Newcomb, Redl, Whyte, etc., and review articles, for example those by Gibb, Platts, and Miller (**57**), Shils (**143**, pp. 40–52), and Swanson (**157**), have little place in a paper of this kind.

Another criterion influential in determining the selection of articles is implicit in the definition of the field of small-group analysis previously stated. That is, for definitional reasons, the work of F. H. Allport, Sengupta, Sinha, Weston, English, Farnsworth, etc. is not considered here. Their work falls under the general heading of "social facilitation" since they were particularly concerned with the way in which the

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[2] Although the great bulk of experimentation has been done only recently, nevertheless this review covers a fifty-year period of small-group research. The first publication date of an article included in this survey is 1902 while studies have been reported up to January of the year 1952.

mere physical presence of others acts as a stimulant on individual activity, eliminating for the most part interpersonal communication. For similar reasons many studies of the effect of spectators or auditors, and of rivalry and competition in human and animal subjects are excluded. Thus, studies which provide the possibility of intragroup communication form the core of the field of small-group research.

The studies of small groups here reviewed are classified in terms of the *independent variables specified* for investigation. The general plan of this survey is to subsume the investigations under five topical headings. These headings are as follows: (*a*) studies designed to contrast and compare behavior of groups and individuals, (*b*) studies which involve the manipulation of social structure variables important to group functioning such as authority relationships, (*c*) studies of the effect of cultural variables, that is, the sharing of values and goals in a group, (*d*) studies involving the manipulation of situational conditions such as group task, size of group, communication networks, etc., and (*e*) studies of personality variables affecting group behavior.

Behavior of Groups and Individuals

Many experiments have been designed to determine whether persons working cooperatively in groups are more efficient at problem-solving than persons working without interaction or in competitive, individually-oriented circumstances. The dependent variable in the typical experiment under this heading is a productivity index, such as number of mathematical problems solved or accuracy of judgment. This was the main concern of the early experimentalists, but interest has waned with present emphasis on processes in small groups.

Efficacy of Group Performance versus Individual Performance

Problems with specified criteria of evaluation. Group discussion seems to have an effect on individual behavior in a wide range of activities and this effect is generally evaluated as desirable by the experimenter. Münsterberg (**120**) found that individual judgments of the numbers of dots on cards were more correct after group discussion than before. Group performance has also been found to be more efficacious than individual performance for word-building out of letters by Watson (**163**); for numbers of arithmetic problems correctly solved by Shaw (**141**), Barton (**10**), and Klugman (**90**); for memory and word construction by Radu (**132**); for accuracy of judgment of events by Klugman (**91**); and for accuracy of judgment of events in a legal situation by Dashiell (**37**).

All of these laboratory studies deal with a simple type of problem for which there are definite criteria to evaluate performance. However, the same results have been obtained using more complex situations. For example, Lorenz (**102**) studied the effect of the work group on productivity in a shoe factory and found an increase in output efficiency of a worker in a cooperating group over his output when isolated.

Granting that group membership has a powerful effect upon individuals, still the conditions under which such an empirical generalization holds must be explicitly stated. For example, Burtt (**28**) repeated Münsterberg's experiment and found little confirmation for the results previ-

ously drawn. Such contradictory findings can only be resolved by carefully designed experiments in which variables and conditions are explicitly and clearly specified.

Marston (**110**), criticizing Dashiell's experiment for lack of specification, showed that a single trained individual may be a more successful finder of facts than a number of untrained jurors, this superiority varying with the specific kind of fact finding in which he has been trained. Also, the knowledge an individual may have of how others' opinions in his group differ from his own is a condition which greatly influences the effective role of argument and persuasion according to Jenness (**83**). His study was designed to qualify the conclusion drawn by Münsterberg that "individual judgments are more correct after group discussion than before." Münsterberg had always arranged it so that the subjects in his experiment knew exactly what the others' judgments were before group discussion began.

Other authors, Gordon (**59, 60**) and Stroop (**155**), point out that in many of the studies in this area, for example Watson's study, the results would have been little altered if each person had worked separately and their products combined statistically. That is, when subjects made independent judgments of weights and these were artificially combined into statistical aggregates, the composite judgments increased in correctness with increased size of the aggregate.

Gordon's criticisms refer to tasks which merely require the addition of individual contributions. Thus, "group" thinking is superior to that of an individual just by the fact that the thinking of a number of individuals is pooled. Under such conditions, as was pointed out by Marston, a particular individual working alone may be superior to a group and would be impeded through group discussion with inferior persons. However, when a task is used which involves a number of logical steps, all of which must be in a correct order to find the right answer, such as in Shaw's study, group discussion may have real advantages over individual endeavor. The advantages seem to be due to the rejection of incorrect suggestions, the checking of errors in the group, and the increment in the range of suggestions and original ideas.

Other versions of the same type of study may be found. Deutsch (**39**) essentially induced an attitude of mind in his investigation of cooperation and competition similar to that created in the foregoing studies. The students of the cooperative groups received final grades that depended upon the solutions to puzzle and human relations problems which their group as a group submitted. In the competitive groups the final grade was determined by how much each individual contributed to the solution. The findings indicated that greater productivity occurs when the members of a group are organized in terms of cooperative activities rather than competitive ones. Evidence supporting this conclusion has been reported by Mintz (**116**), Stendler, Damrin, and Haines (**151**), and Maller (**105**). Maller concluded that cooperation with an organized team resulted in greater efficiency than work for the self, while cooperation with an arbitrary group, chosen by the examiner, resulted in lower efficiency. Efficiency was measured by the speed with which simple additions were done by school children.

Problems with unspecified criteria of evaluation. Group discussion has been shown to have an important

influence on individual behavior even where performance can be judged, only arbitrarily, as more correct, faster, or more effective. Bechterew and de Lange (**18**) gave subjects moral conflict situations to decide upon and found that after group discussion individual solutions were more inclusive of the many relevant facts in the case as well as more similar to each other.

Sherif (**142**), using the autokinetic phenomenon, had subjects, while working alone, make successive judgments of the extent of the apparent movement of a light. Under these conditions each subject developed a range within which he made his estimates. These subjects, when subsequently put in groups of two and three, gradually came to make their judgments within a restricted range, characteristic of the group. When, however, subjects began by making judgments together, they kept their group norms while later making judgments alone. Sherif uses this experiment as the prototype by which group norms, attitudes, and values are established. Propagation of similar type experiments has occurred (**24, 64, 140**).

These latter studies, although not posed in efficiency terms as the former experiments, are likewise designed to prove that a group situation, vaguely defined, does indeed affect individual performance. Such studies characteristically minimize the group discussion process itself, the analysis of which is the core of small-group research at present.

Efficacy of the Lecture Method versus the Group Discussion Method

Essentially the same question is being investigated in the work aimed at proving that group discussion has more powerful effects upon individual behavior than the traditional lecture method. In the actual teaching of a college subject by the lecture and class discussion methods, Jones (**87**) found that students scored higher on immediate and delayed tests for reproduction in the discussion groups. This result was substantiated only for delayed recall by Bane (**9**), however, and Spence (**148**), working with graduate students, found just the reverse result to that of Jones. Recently Husband (**79**) and Asch (**3**) have reported that the lecture method is more efficient in teaching than the method of small section meetings when final grades are used as the measure of achievement. However, Asch drew just the opposite conclusion, using measures of change in social and emotional adjustment as criteria.

Many of these contradictory conclusions are no doubt the result of poorly specified and poorly controlled experimental variables as well as the lack of uniformity in the operational meaning of "efficiency." The conditions under which group discussions are found to be superior to lecture methods ought to be explicitly stated.

Other experiments have been conducted in a variety of situations. The results are unequivocal in the studies of food habits by Lewin (**95**), Guthe (**65**), Willerman (**166**), and Radke and Klisurich (**131**). More housewives changed their behavior and attitudes about various types of foods after participating in a group discussion than after hearing lectures concerning these foods. In the lecture situation individuals resisted anything that might have made them depart from old group standards which they had internalized. After group discussion, however, change was facilitated since new group standards had evolved. Thus the

resistance which was due to the relation between the individual and the group standard was eliminated.

In addition, group discussion has been found to be effective in changing a wide range of behavior patterns. Prejudicial attitudes have been changed (**99**, **109**), hostile attitudes lost (**20**, **86**), community problems solved (**80**, **98**), alcoholics have been cured (**5**), neurotic disabilities alleviated (**22**, **89**), emotionally disturbed children helped (**21**), productivity raised (**35**, **81**), roles and status changed (**125**), frustration induced most successfully (**97**), and disabilities accepted (**41**). We need not be further persuaded that group discussion processes have an effect on individual performance even though there is a selective process occurring in the reporting of studies. This proof has only opened up new and troublesome problems concerning the mechanisms by which this influence is achieved and the conditions under which such an empirical observation holds.

In the food-habit studies, for example, there were certain conditions which, needless to say, determined the results. In these discussion groups there was no question who the discussion leader was, and he functioned in that role from beginning to end. Furthermore, there was no question about the objective of the group discussion. It was designed to change the food habits of the members. These are but two of the possible factors which conditioned the results.

Many of the other studies lose considerable persuasive powers *by failure to make use of a control group design*. Also, the experimental variable, vaguely defined as "group discussion," has included groups with leaders or without, groups of various sizes, meeting over different periods of time, some with opportunity for feedback and self-evaluation, some permissive and others directive, groups with different purposes and with all types of participants, from children to neurotics. Finally, the results of these studies have not been as cumulative as might be expected since the problems set for investigation tend to be structured in a polemical way.

Social Structure Variables

In this section, experiments are reported which attempt to make explicit the effects of one set of variables, variables which were uncontrolled and for the most part unrecognized in the previous section. More specifically, different types of authority relationships external to the group are investigated in terms of the effects these have on group productivity, shared attitudes in a group, therapeutic gains, group integration, amount of communication, etc.

Diffuse Authority Relationships: Studies in Attitude Change

In the process of interaction, group members develop norms, attitudes, and motives which they hold in common, according to Whyte (**165**). These norms may or may not be in accord with the norms of persons in authority positions. The following studies indicate the consequences on small-group behavior of two status levels holding concordant or divergent norms, goals, and interests about a common subject.

The pioneer studies in industry by Mayo (**111**), Roethlisberger and Dickson (**135**), and Whitehead (**164**) of two small groups of factory workers indicated that when strong loyalties exist between workers and manage-

ment, productivity is high but when negative sentiments prevail, the workers develop an elaborate informal system of rules and sanctions such that output is actually restricted. Here loyalties toward management were established by the workers by virtue of their selection for special participation in the studies, and of management's policy not only to consult these special workers before any change was introduced into the work routine but also to allow the workers to participate in the designing of the new job. Negative sentiments were engendered as the result of management's disregard of the workers in policy making.

Similar results have been reported to point up the advantages of cooperative action on the part of management with workers (**2**; **14**, p. 25; **51**, **52**, **108**, **109**). In addition, resistance of piecework employees to changes in their work methods and jobs prescribed by management was found by Coch and French (**35**) to be inversely related to the amount of joint decision-making granted to workers by management. With active participation, the workers developed task goals among themselves and with management and overcame their resentment against authority so that successful job change was possible. Maier (**104**) showed further that a leader skilled in conference procedure and possessing exceptional ability in solving technical problems could conduct a discussion so as to obtain not only group acceptance of his ideas but also to obtain a quality of problem-solving that surpassed that of a group working with a less skilled leader and/or without technical competence.

This result has been found not only in industrial settings. McCandless (**113**), working within a training school setting with high-grade, mentally deficient children and predelinquents, wanted to decrease the social acceptability of the domineering type of leader in the informal peer groups. He found that the relationship between dominance and popularity was high in cottages directed by adult supervisors, but that this relationship decreased as the boys themselves were allowed to participate in supervision and policy making. Preston and Heintz (**130**) found that "participatory" leadership as opposed to "supervisory" leadership was more effective in changing attitudes toward presidential nominees, in producing group agreement, and in making the task more interesting among college students.

Specific Authority Relationships: Studies in Leadership Style

The studies reported in the previous subsection were phrased in terms of changing attitudes of group members in order to affect some other desirable goal. The following studies have to do with leadership style which can be manipulated to induce different types of specific authority relationships in order to study its effects on other small-group behavior.

The very famous Lewin, Lippitt, and White (**96**, **100**, **101**) study of autocratic, democratic and laissez-faire leadership is an excellent example of this type of experimental manipulation. The autocratic leader determined all policies, techniques, and activities. He maintained his autonomy by remaining aloof from active group participation except when demonstrating to the group members what they were to do. In the democratically led groups all policies were determined by group discussion with the leader taking an active role. In the laissez-faire

groups, the leader took no active part, the group having complete freedom for group or individual decision.

The leaders, by virtue of their own behavior toward the groups, induced response patterns not dissimilar to the results obtained with industrial workers. The autocratic leaders provoked hostile behavior among the boys (cf. Mayo and Lombard [112] in the next section) or apathetic behavior (cf. Mayo [111]). The laissez-faire leaders created interpersonal friendliness among the group members but dissatisfaction with the task was high for these groups. The democratic leaders were preferred in a popularity rating. Their groups were task oriented, well satisfied with their achievements, and highly integrated. In terms of quantitative work output, the submissive autocracies surpassed the other groups. However, the products of the democratic clubs are reported to have been qualitatively best because of "the close attention given to every detail."

Nevertheless, the proselytizing of a faith in democratic leadership is not the scientific aim of small-group study as here defined. The scientific aim is rather to delimit the conditions —social, cultural, psychological, and situational—under which this empirical relationship holds. For example, would the results be confirmed in an authoritarian organization like an army? Only with careful definition will results contribute to the cumulative growth of small-group study.

Thelen and Withall (**159**) have made a contribution to this aim by investigating the differences in perceptions by group members under various styles of leadership and consequently the extent to which leaders can indeed induce desired experimental conditions. They found that gross structural characterizations of leadership styles by persons possessing very different bases for evaluation (these bases were labelled objective-behavioral, projective-attitudinal, subjective-internal) were in substantial agreement. Heyns (**72**) observed the effects on participant behavior of discussion leaders who manifested an active and positive relationship to their groups as contrasted with leaders who had essentially negative attitudes. The most notable result was the greater incidence of supportive and cumulative contributions among members in the positively led groups and of opposing contributions in negatively led groups. Also there was greater acceptance of opposing behavior in the negatively led groups such that opposers received high popularity ratings and were the most highly satisfied with the decision.

Other studies of leadership style in small discussion groups have been conducted in an educational setting with the purpose of improving teaching methods. The results at present seem to be inconsistent. Wispé (**167**) used two styles of teaching in discussion groups at the college level. He found no differences in final examination marks between students in directively led groups and students in permissively led groups. However, students preferred the former. On the other hand, Allport (**1**) reported results to indicate that students preferred informal, permissive, and friendly discussion groups, as did Gross (**62**), who also found significant gains in insight on a self-insight scale administered before and after the group meetings. Evans (**42**) concluded that directive leadership in therapy groups is more efficacious than nondirective leadership, using

therapeutic gains, member preference, and attendance records for the basis of this conclusion. On the other hand, Faw (**43**) found that the nondirective approach was preferred by students, that it induced high student participation and better performance on examinations. Bovard (**25, 26**) showed that affectivity was higher between members in "group-centered" groups and members were more susceptible to opinion changes than in "leader-centered" groups. The advantages of "group-centered" groups in school learning have been pointed out by Flanders (**46**) and Perkins (**126**). Rehage (**133**), however, obtained results which did not warrant claims of superiority for one method over another.

Put in *efficiency* terms these results are confusing. (The same contradictory conclusions were encountered in the polemical studies of the efficacy of the lecture method versus the group discussion method.) Like many evaluative studies, the conditions under which advantages are greater for one type of leadership style than for another are for the most part poorly stated. The experimental results of these studies stand as particular findings, not unified into a conceptual scheme of problems and concepts of social relationships, and incapable of such unification because there is lack of systematic study of the effects of independent variables.

Recent empirical and theoretical writing (**6, 7**) about small-group behavior has tended to regard the small group as a dynamic system of action, which ebbs and flows between instrumental-adaptive activity as task problems are being solved, and expressive-integrative activity as socio-emotional problems among the members are attended to. That is, over and above the common norms and attitudes which are built up in the group, there are equilibrium properties of the small group itself such that imbalances of activity in certain areas have repercussions in other areas of activity.

Evidence for such a view has been presented by Sterling and Rosenthal (**152**). They found that leaders and followers change with different phases of group process, the same leader usually coming to the fore as similar phases of the group recur. Bales and Strodtbeck (**7**) present evidence to support the notion of phases in group problem-solving. Under certain conditions there are subperiods within the total period, in which interaction changes its character in predictable ways as a group proceeds from initiation to completion of a continuous period of interaction. A similar view is held by Heyns (**71**) and Plank (**127**).

With such a conceptual scheme, perhaps questions posed in dichotomous terms, such as directive or permissive leadership styles, are not formulated most realistically. The behavior style of a leader may be appropriate for one phase in the sequence of events but maladaptive for another. If a leader rigidly maintains one behavior pattern and is not sensitized to his functions in the various phases of the group, then something less than maximum group satisfaction or efficiency will conceivably ensue.

A similar criticism can be levelled at many studies of sociometric patterning and composition of groups, in which the assumption is made over and over again, for example by Zeleny (**169**), that compatible groups are the most productive or efficient groups. By maximizing the expressive-integrative dimensions in a small group it is assumed that this will *ipso*

facto facilitate the solution of instrumental-adaptive problems. If workers are given the opportunity to express their needs, their productivity rates will go up. This type of static, one-sided thinking is not consistent with the conceptualization of a small group as a dynamic system of action and has not been supported by empirical evidence (**136**).

In two remaining studies by Thibaut (**160**) and Kelley (**88**), the experimenter, rather than acting in accordance with a specific leadership style, differentially meted out rewards to young boys working in teams. In both, a team could receive one of four experimental treatments: it could consistently receive reward throughout the experimental period; it could receive consistently no reward; it could receive reward at the beginning of the experimental period but by the end be receiving no reward; and finally, it could gain in reward from the beginning to the end of the experiment.

Thibaut formed groups in such a way that at the outset each group had approximately half of its sociometric choices in its own group and the remaining half in an opposing team. He studied the effect of the experimenter's manipulation of rewards upon the proportion of own-group choices. Only with consistent reward or no reward, or what Thibaut called high and low status, did the sociometric attractiveness of one's own team increase. He also found that the amount of communication initiated by a group tended to increase as the group's position changed toward a lower status and tended to decrease as the team's position became more rewarding. The hypothesis presented was that people who are motivated toward upward mobility will tend to communicate with those above them in the organizational structure.

Kelley concluded that the most disruptive circumstance to intragroup friendliness was what he called high status (reward) combined with the possibility of demotion (withdrawal of reward) and low status combined with the impossibility of promotion. The provision of status security for the highs or the addition of the possibility of moving upwards in the case of the lows led in his groups to the maintenance of group solidarity. He also found that the very existence of status differentials may operate to prevent ease of communication.

This concludes the section on types of authority relationships and their consequences on small-group behavior. One of the dependent variables in these studies is the presence of common attitudes, norms, and values among group members. In the next section, the degree to which a culture is shared in a group becomes an independent variable which is manipulated in order to study its effect on other aspects of small-group behavior such as problem-solving, amount of communication, content of discussion, reaction to frustration, etc.

Cultural Variables

The cultural component in small-group experiments has been manipulated in two different ways. First, group affiliation has been used as an indication of shared values and norms on the assumption that these are formed and held in common as group members interact with one another. Group affiliation is a difficult variable to use, however, since the degree and type of acquaintanceship are often hard to specify. Consequently, a second method has been devised. Groups are formed and in-

structions given in such a way as to establish group frames of reference desired by the experimenter.

Unfortunately, the strengths of one method of variation are the weaknesses of the other. The second type of variation has advantages in that the extent to which norms and goals are shared can be precisely specified. It has the disadvantage of creating experimental situations in which group affiliations are artificial, goals limited, obligations weak, and permanency lacking. The reverse is true of the first type of variation. Thus, the study of the effect of a common culture has inherent difficulties in an experimental setting.

Group Affiliation

There are a number of studies which investigate the effect of prior acquaintanceship and therefore of shared values on other small-group behavior. In a study by French (**49**, **50**) the organized groups were composed of teammates from a neighborhood house while the unorganized groups contained subjects who were unknown to each other before the experiment. Both groups were exposed to experimental conditions of frustration, produced by the attempt to solve objectively insoluble problems, and to conditions of fear produced by suffusing their locked room with smoke in simulation of a fire. The results indicate that the members of the organized groups compared with the unorganized groups were more interdependent, had greater we-feeling, had greater equality of participation among members, had greater motivation to complete the problems and greater feeling of frustration when unable to do so, were more inclined to direct their aggression toward other persons in their group, and showed more fear. Using preschool children, Wright (**168**) studied the social behavior of pairs of strong friends and pairs of weak friends when subjected to frustrating situations. The results indicate that strong friends showed more cooperation and more aggressiveness than weak friends.

Using two behavioral indices—absenteeism and turnover—Mayo and Lombard (**112**) investigated differences in group solidarity between two large industrial plants. The factories were similar in geographical location, technology, and labor force but differed in the degree of primary group organization on the worker level. In the low-turnover factory, personal bonds, shared norms, constitutive of a primary group, were well established and newly recruited workers were easily incorporated. In the high-turnover factory, these bonds were weak and no sanctions were enforced as workers performed their functions independently of one another. This was especially true of the new workers who showed a very large proportion of absence and turnover. This result has been substantiated by Fox and Scott (**48**) and Davis (**38**) among others.

Strodtbeck (**154**) studied husband-wife dyads in three cultures as they attempted to resolve opinion differences. The cultures differed in the way the status of women was defined, women having a powerful position in Navaho society compared with men, less powerful in the Texan community, and least powerful in the Mormon group. He found that the differential ability of husbands and wives to win decisions and to participate verbally in the experimental

sessions was closely related to the definition of the power positions of men and women in the larger social and cultural organizations. He pointed out the further relationship between the actual amount of influence exerted by a person on a group decision and his pattern of participation, a result substantiated by Staton (**149**). The most influential spouse tended more frequently to ask questions, carry out opinions and analysis, and make rewarding remarks while the less influential spouse showed acts of agreement, disagreement, and aggressive acts designed to deflate the other's status. The wide range of possibilities that may be used to influence the behavior of another has been described by Merrill (**115**), who observed mothers and children in a standard play situation.

Finally, Gyr (66) attempted to obtain descriptions of the cultural differences in customary committee procedures by interviewing representatives from China, South America, the Near East, and the United States. The findings are extremely tentative due to the inadequacy of the sample. The greatest differences are evident in the attitudes of South Americans and North Americans. There appeared an awareness of leader superiority, trustfulness in delegating authority, and desire to cooperate in the United States sample, while in the South American sample there was general uncertainty about the motives of others.

Experimental Instruction and Arrangement

There are several studies in which groups were formed and instructions presented in such a way as to create low or high group affiliation or cohesiveness. "Cohesiveness" is used in these studies as an explanatory concept.

Back (**4**) used differences in the salience of instructions to establish low and high cohesive groups. It was found that in the highly cohesive groups, members not only tried harder to reach agreement and to influence their partners on controversial issues but were somewhat more willing to accept their partners' opinions than were members of the low cohesive groups. Schachter, Ellertson, McBride, and Gregory (**139**) created low and high cohesiveness within groups in a similar way in order to determine what effect cohesiveness had on standards of production. There was no indication of any necessary relationship between cohesiveness and productivity. However, attempts by the group to influence a member to decrease her rate of production was more effective in high than in low cohesive groups. Cohesiveness appeared not to be a determining variable when the request was made to increase output.

Festinger and Thibaut (**45**) manipulated the solidarity dimension by the force with which the experimenter emphasized in the instructions the necessity, first of all, for a task solution and, second, for a unanimous group decision. The results indicated that the volume of communication directed toward a group member was a function of his holding an extreme opinion. The greater the solidarity, the greater was the tendency to communicate to persons expressing extreme opinions and the greater was the actual change toward uniformity of opinion in the group as a whole.

Finally, Schachter (**138**) created his degrees of cohesiveness by manipulating the attractiveness of the

activities which the groups mediated. That is, students becoming members of clubs of their choice constituted high cohesive groups while those becoming members of clubs which they were not interested in joining made up the low cohesive groups. Using stooges to express deviant opinions, he showed that in high cohesive groups, the deviant was more strongly rejected on sociometric testing than he was in low cohesive groups. In terms of amount of communication addressed to the various group members, he found that more communication was addressed to the deviant in highly cohesive groups, and that this trend was more pronounced during the discussion of issues vital for the group.

Thus, group affiliation and the sharing of norms and goals by members seem to have behavioral correlates. Group affiliation and its concomitants were defined independently in the former studies, while in the latter studies (with the exception of Schachter) cohesiveness was inferentially derived. These latter studies failed to create many of the characteristics of primary group membership. They tended in that direction to the extent that the group members were influenced in their attitudes in the way predicted by the experimenter; that is, to the extent that the group members accepted the definition of the situation and of the goals pronounced by the experimenter. The experimental conditions seemed to achieve an approximation which went far in effectively changing participant behavior in the expected directions such that it appeared justifiable to include these studies in this section. The circularity in this argument is apparent, and a closer examination of the concept of solidarity or cohesiveness created by experimental manipulation is required.

Situational Variables

In this section, studies are included which are designed to define more precisely the influence of variables which are part of the physical situation impinging on a group. Four such variables have been studied: (*a*) the nature of the task presented to the group, (*b*) the numerical size of the group, (*c*) the spatial arrangement of members, and (*d*) the external restrictions on communication channels.

Task Problem

Every conclusion made about small-group behavior depends upon the instrumental or task problems confronting the group under study. Generally the task problem itself is not the independent variable but is a situational condition which is included in the interpretation of results, or is disregarded and unspecified.

There are several experiments actually designed to determine the nature of task influences on small group behavior. Carter and Nixon (**31**) used three different tasks—intellectual, clerical, and mechanical-assembly—as situational variations which were shown to affect leadership behavior. In general, the subjects who took the lead and influenced their partners in the intellectual task also influenced their partners in the clerical tasks. The mechanical-assembly task gave only low intercorrelations with the other two problem-solving situations. In a later study Carter, Haythorn, and Howell (**29**), also interested in the relationship between leadership and group task, employed factorial analysis, a type of analysis used in small-group

study, particularly by Cattell (**33**) in his search for general group and leadership dimensions. Two major factors were revealed—an intellectual leadership and a leadership based upon manual skills. The conclusion to these studies was that leadership is specific to the situation. However, as pointed out by Gibb (**56**), this does not exclude some degree of generality of leadership in similar or related situations.

Deutsch (**40**) found that the differences in task structure between mathematical problems and human relations problems determined to a great extent the quality of group process. In the solution of the mathematical problems, there was more individualized effort, less coordination of efforts among group members, fewer attempts at communication, and more communication difficulties. The content of the human relations problems was more "value-laden," which provoked more conflict in the group, that is, blocking, self-defending, and aggression among the members. The previously cited study by Shaw (**141**) likewise revealed major differences in group process contingent on the task presented for solution.

Heise and Miller (**67**) used three kinds of tasks: (*a*) a simple reassembling of a list of standard words, (*b*) construction of a sentence, the words of which had been distributed among the group members, and (*c*) anagram formation. Using controlled communication channels between members (see the subsection on Communication Patterns) they concluded that the relative efficiency of a communication pattern depended upon the kind of problem the group was trying to solve. While the network pattern of communication was an important condition in the solution of the reassembly and construction problems, it had little effect on anagram formation. The reassembly task was most efficiently solved in groups where all members could talk and listen to all other members. The construction problem, however, was solved most efficiently in a group which had a man in a central coordinating position.

Finally, Bales and Strodtbeck (**7**) found that the sequence of events in problem-solving groups varied in character with the type of problem under consideration. The tasks they analyzed ranged in substance from group projection sketches of Henry and Guetzkow (**70**) and chess, to group decision and planning problems with various degrees of reality.

Thus the problem presented to the group has been shown to be a pervasive situational condition which affects small-group behavior, variously measured by, for example, leadership, efficiency, and sequential scores. So far, few studies have investigated the task problem as an independent variable. What results are available sound a cautionary note for all small-group experimenters to specify clearly the problem orientation of the groups under investigation and interpret results accordingly.

Size of Group

Very few experiments have actually been designed to discover the differences attributable to change in group size, despite the very early intuitive analysis by Simmel (**144**) of the effect of group size on the sociological form of the group. Moede (**118**), using the physical pulling power of men as his measure of effectiveness, found that a four-man group was most economical. He claimed that the pulling power of the average individual of a group de-

creased by 10 per cent with each additional worker. Marriott's (**107**) study of a total of 330 various sized management-organized working groups in two motor-car factories demonstrated an inverse relationship between output per man and group size.

However, as was pointed out by South (**147**), the efficiency of various sized groups depends upon, among other things, the type of problem the group is attempting to solve. Comparing three-person and six-person groups, he concluded that the smaller groups are more valuable when the problem lends itself to immediate formation of solution while the large groups perform more efficiently when the problem requires that wrong hypotheses be promptly rejected. The other factor he specifically investigated as affecting efficiency was the sex composition of groups. He concluded that mixed groups are less efficient. However no systematic treatment has been given to this variable even though most experimenters make implicit assumptions about its effect by purposely studying either all male or all female groups.

More recent investigations have not been framed in productivity terms but in terms of the effect of size on social structure. Hemphill (**69**) concluded that within the limits imposed by his methodology, the size of the group is a variable which to some degree conditions leader behavior. As the group becomes larger, demands upon the leader's role become greater and more numerous, and tolerance for leader-centered direction of group activities becomes greater. Bass and Norton (**12**) concluded that the relative stratification in a group, measured by the mean and variance of leadership ratings made by observers of participants, tended to increase with increases in discussion group size. Essentially the same result has been found by Bales, Strodtbeck, Mills, and Roseborough (**8**), who have reported that the difference in total amount of participation between the most talkative person and the least talkative person in a group increases as the group size increases from three to ten men. There is need for further specification of the effect of this important situational condition on the behavior of small groups and greater sensitivity to its influences.

Spatial Position

Working on the assumption that a person will be more likely to interact with another if he is in a good position to see what the other is doing as well as hear him, Steinzor (**150**) investigated the spatial factor in face-to-face discussion groups. He found that the degree of interaction among the members sitting more nearly opposite from one another in a circle differs significantly from that expected by chance. As an ecological factor, spatial position is not unrelated to networks of communication, to be discussed in the next section, or to propinquity as a determiner of friendship patterns in the community at large (**44**).

Communication Patterns

A situational variable being investigated by a team of researchers headed by Bavelas (**15, 16, 17**) is the communication network. The standard situation used is one in which the communication paths between members of the group are controlled so that the effect of predetermined patterns of communication can be studied. Communication is usually in written form. The standard task presented to the group consists of the

simple collection of information, each member contributing some components to the correct solution. The task as a conditional factor in the interpretation of the results has been studied by Heise and Miller (**67**). (See the subsection on Task Problem.)

Leavitt (**92**), using four communication patterns—circle, chain, **Y**, and wheel formations—found that the circle and wheel patterns produced the most contrasting results. Using, as measures of effect, records of speed, errors, and number of message units in the solution of the problem, as well as postmeeting questionnaires, it was found that the circle pattern produced the most active, erratic, unorganized, leaderless but satisfied group. On the other hand, the wheel pattern was least erratic, required relatively few messages to solve the task, was organized with a definite leader, but was less satisfying to most of its members. The member in the central position in the communication network became the leader and he was more satisfied with his job than were the occupants of peripheral positions. The various communication networks did not, however, differ significantly in the average time taken to solve the problem. Similar results were obtained by Smith (**146**, p. 197), who used the same communication patterns. In addition he found that the circle pattern permitted the members to adapt more readily to a change which required the relearning of certain parts of the task and which upset a previously established learning set.

Heise and Miller (**67**), using networks of telephone channels, varied the intelligibility of messages by manipulating the relative intensities of speech and of extraneous noise. They found that by exposing a group to such unfavorable intelligibility conditions the differences in the relative efficiency of different networks can be exaggerated.

Again using the Bavelas technique, Leavitt and Mueller (**93**) showed that a two-way communication circuit between the sender of information and the receiver or executor of information, a condition which they called "feedback," increased the accuracy with which information was transmitted as well as increased the confidence of the receiver and sender in what they had accomplished. Although the condition of no feedback was less time consuming, it engendered hostility in the receiver and doubt in the sender.

The experimental situation investigated by this group of workers is considerably more restricted than most group situations studied in the laboratory. The differences between communication networks are maximized while it is assumed that other antecedent conditions—social structural, cultural, personality, and situational—are for the most part held constant. The development of a common culture between members is minimized by the nature of the standard situation and by the nature of the task which involves merely the collection of information. Any conclusions drawn from these studies must be in terms of the carefully controlled conditions under which the groups must work. This is a powerful experimental method if precision is not achieved at the expense of too little applicability.

Communication patterns have also been studied in free discussion situations by Bales, Strodtbeck, Mills, and Roseborough (**8**). Certain average empirical tendencies have been found. For example, he who initiates most action will receive most and will

address more of his remarks to the group as a whole than to specific individuals, etc. Therefore, even though the communication pattern in a free discussion group is uncontrolled at the beginning, there is a tendency for a pattern to be formed in the course of the group meeting such that certain channels have a higher probability of use than others. Thus the results about communication patterns derived from this treatment and the paradigm of Bavelas *et al.* seem capable of generalization.

Personality Variables

The investigations included under the heading of "Personality Variables" are more often than not correlational studies which relate personality factors such as attitudes, needs, abilities, dominance, masculinity, talkativeness, etc. to measures of leadership in a group, interaction patterns, clique membership, and social status. The aim has been to study the internal dynamics of group behavior or some of the endogenous elements in the group deriving from the personal characteristics of the members and their roles within the group.

While previous investigations have studied the consequences of independent variables on the group as a whole or on problem solving in general, these studies focus rather on more microscopic effects. Personal material about members is used both for independent variation and for the measurement of effect. This double-barrelled interest in personality factors has tended to foster the disregard of the other factors which influence behavior in groups, such as social structure, culture, and situation. This neglect of other conditions in the design of experiments has led to faulty predictions and to overgeneralization.

The majority of the studies have to do with leadership behavior within groups. The previous studies of leadership were classified under the social structure heading since these concerned authority positions external to the small group.

Studies of Leadership Behavior

Interest in the problem of leadership has been keen for years. Since there are excellent reviews of the literature available (**68, 82, 153**) and since the main concern here is with small-group research, a great bulk of the material will not be mentioned.

One of the oldest polemics in this area of study is whether personality qualities are the pervasive factors in the determination of leadership, qualities which some people have and other people have not, or whether leadership is situationally determined. There is much evidence for the former view. Bell and French (**19**) attempted to determine the extent to which individuals maintain consistent leadership status in a series of informal discussion groups made up of different members. At the end of each session, the members of each group were asked to nominate a discussion leader for a hypothetical second meeting. On the basis of these results, the writers concluded that leadership status was highly consistent despite the situational changes involved. Of course this finding is relative to the other conditions operating in this study such as the size of groups, the type of task, permanency of group, population sample, etc., as has been pointed out before. However, the same consistency between leadership as rated in one situation and as rated in another was found by the OSS staff (**123**) in assessing officers during World War II. There were seven

social situations used for assessment: an interview, a problem-solving situation, a construction problem situation, panel discussion, debate, assigned leadership situation, and ratings by associates.

Merei (**114**) studied the extent to which children with leadership ability, so designated by nursery school teachers and observers in this study, had to change their behavior when placed in groups of children who had formed traditions and rituals. The results indicate that the leader adopted the new group's traditions rather than instilling his own. While in this sense he proved to be weaker than the group, he still managed to play the role of leader.

If leadership is consistent, what are some of the qualities that leaders possess? Attempts at specifying leadership qualities have been made by Chapple (**34**). He has devised a method for selecting supervisory leaders which has been employed for a number of years with apparent success in business organizations. Chapple assumes that an individual has a constant relationship between the frequency and duration of his verbal actions and inactions regardless of the situation. He records these interaction units in an interview on a polygraph which he calls an interaction chronograph. By comparing the pattern of interaction of a prospective supervisor with the interaction pattern developed to define the requirements of a particular supervisory position, he selects and places candidates. Chapple is inconsistent in his argument to the extent that he recognizes that different situations require different patterns of interaction, but he still tries to generalize from a two-person interview situation to all others. His assumption about the constancy of individual participation patterns loses force in the face of other evidence.

Using a method of recording similar to Chapple's, Horsfall and Arensberg (**77**) observed leadership patterns in teams of industrial workers. Although teams were structured around a highly interactive person there was no uniform rate of interaction within a team over and above the finding that the more productive teams had a smaller amount of "within" team interaction. Leaders showed a combination of a high interaction rate between and within teams, rather than a high rate of "initiating responses" (as distinguished from "terminating responses") or a particularly high overall interaction rate.

There are other investigations which have been designed to delimit and define leadership qualities. A correlational study with young children in nursery school groups was reported by Goodenough (**58**). She found correlations of the order of 0.60 among five variables—physical activity, talkativeness, laughter, social participation, and leadership. Bass (**11**) had judges rate participants in unsupervised discussions on a series of 13 items designed to reveal leadership status. These ratings were correlated with the time each participant spent in talking in the group session. Coefficients ranged from 0.82 to 0.92. In another study, Bass and White (**13**) had fraternity members judge each other on leadership items after which they participated in free group discussions. The ratings made by trained observers of the participants were found to be highly related to the judgments of members. Using similar experimental conditions for his study, French (**53**) reported confirmatory results. Green (**61**)

pointed out the further relationship between effective group participation, measured by duration of participant's contributions, and verbal intelligence measured by standard verbal intelligence tests, while Brown (**27**) reported high significant correlations between total participation and high manic scores on the Minnesota Multiphasic Test.

Hemphill (**68**) used questionnaires in studying leadership qualities. Respondents acted as observers of groups of which they were members. Each respondent gave a description of his group and reported his observations of the leader's behavior. The behavior required of all leaders regardless of the situation in which they functioned had the following characteristics: (*a*) ability to advance the purpose of the group, (*b*) competence in administrative functions, (*c*) ability to inspire the members of a group to greater activity or to set the pace of the group, (*d*) behavior which added to the individual member's feeling of security in his place in the group, and (*e*) behavior relatively free from activities serving self-interest.

Finally, in a field study of children in summer camp, Polansky, Lippitt, and Redl (**129**) found a positive relationship between leadership in a group (sociometrically determined) and the extent to which a person will directly attempt to influence the conduct of others and in turn show spontaneous reactions to the behavior of others. These data stimulated a further laboratory study by Grosser, Polansky, and Lippitt (**63**) of behavioral contagion and the qualities important in the acquisition of group prestige. They found that a stooge was able to influence the behavior of another just by his own acts of initiation.

A number of studies have already been presented, however, which demonstrate the importance of situational factors in the determination of leadership behavior (**8, 31, 29, 69, 92**). In addition, Hollingworth (**74**), in a field study of children's groups, found that children with high intelligence were seldom leaders in groups of average children but were likely to be leaders in groups of superior children. Carter and Nixon (**32**) related leadership ratings by school teachers and fellow students to observations of the same subjects participating in small leaderless groups. They found that power-seeking, money-oriented, persuasive, masculine people were often rejected as leaders by supervisors and associates, while in leaderless group situations they became leaders. This provoked a further study by Carter, Haythorn, Shriver, and Lanzetta (**30**) of the differences between leaders who are appointed in and those who emerge from a free discussion group. Although having the same amount of leadership ability, the leaders who emerged were found to be more authoritarian than leaders who were appointed.

Thus personality factors are of prime importance as conditions influencing the behavior of leaders or potential leaders. However, they are but one set of a complex of conditions which interact with one another to determine the behavior which is eventually manifested, the other sets being social structural, cultural, and situational factors. Consequently, any attempts to make predictions from personality qualities, without other reference, to leadership behavior in small systems of social action, have a high probability of failure (**117**). Research which clearly specifies the interaction of factors

holds great promise for small-group study. Several such investigations concerning leadership behavior are available. One of these is a study by Maas (**103**).

Maas was primarily interested in the interrelationships between personality and situational variables affecting the perception and behavior of group leaders. The personality of leaders was described in terms of need structure. Leaders with needs to change others rather than self he classified as projective, and those with needs to change self rather than others as introjective. He used autobiographical material and the California Personality Test as the basis for this classification. The groups varied in structure, the informal groups having open membership with activity programs, and the formal groups having closed membership with social issue programs. He found that projective leaders perceived the behavior of others and acted toward others in a causal and objective way in the informal groups where their needs could be satisfied, but in an evaluative and judgmental way in the formal groups where their needs were not met. The introjective leaders behaved in just the opposite way. That is, they were biased in the informal groups in their perceptions and actions and were objective in the formal groups.

Swanson (**156**) likewise was clearly aware of the interaction of personality and situational factors in the behavior not necessarily of leaders, but of members belonging to discussion groups. The Blacky Test provided personality data, and observations of the group prior to and during the experiment were used as situational data. The groups differed in degree of internal heterogeneity of social status and of ideology about group functioning as well as in the permissiveness of intermember behavior. Given the predispositions reflected in a particular Blacky Test profile, and granted the kind of interpersonal situation that a particular group provided, Swanson was able to predict significantly such measures as a person's volume of participation in the group, the amount of received disagreements from others, the amount of positive affect towards others expressed on sociometric tests, the judgment of others' task contributions, and the degree to which self was perceived as being influenced by others.

The behavior which has been predominantly studied so far has been generally referred to as "leadership behavior." However, the concept of leadership has had little uniformity in operational meaning. It has had such diverse definitions as "nomination as a discussion leader for a hypothetical second meeting," "designation by nursery school teachers," "those in supervisory positions," "ratings by judges in unsupervised discussions," "ratings by fraternity members," "sociometric status," "appointed leaders," etc. The lack of correspondence between such definitions has been demonstrated by Polansky, Lippitt, and Redl (**128**). Also the problems asked about leadership behavior have been posed generally in polemical terms. Consequently these studies do not provide, for the most part, a substantial, well-tested body of supporting evidence, despite the great wealth of experiments. Future investigators may well be advised to seek relationships between operational measures of the behavior of group members without recourse to a poorly specified, nontechnical term such as leadership.

Studies of Social Interaction Patterns

There are ever-increasing numbers of studies which attempt to relate in a systematic way, phenomenological or projective personality data, with interpersonal reference, and social interaction patterns. These are mainly correlational studies in design, little attempt being made at either manipulation or prediction.

A very early experiment by Riddle (**134**) shows the subtle interaction of individual motivation, evaluation, and betting behavior in a series of poker games. Riddle reached the conclusion that the "desire to win" in this game situation was only to a slight extent aroused by the size of the player's own hand value. It was aroused more fully by the value of the opponent's hand. The influence of one's own hand was to increase one's own bet, and the effect of the opponent's hand was to inhibit one's bet. When the balance had been struck and the bet made, this bet in turn duly determined the total strength of the "desire to win."

Coffey, Freedman, Leary, and Ossorio (**36**) and Ossorio and Leary (**124**) found relationships between the social interaction patterns of members in a group therapy situation and their personal ideologies obtained from test and autobiographical data. Changes at one level of personality structure were accompanied by changes at the other as the therapeutic sessions progressed.

Fouriezos, Hutt, and Guetzkow (**47**) reported that ratings of self-oriented needs from clinical material (Rorschach, TAT, sentence completion, and interviews) and from observations of persons in discussion groups were highly correlated. The self-oriented needs used by these investigators were dependency, status, dominance, aggression, and catharsis. They also found that self-oriented needs correlated negatively with group satisfaction and positively with the amount of group conflict. Malone (**106**) observed group psychotherapy sessions over a 12-month period. Therapeutic success, measured in terms of individual improvement, showed marked ups and downs during the observational period. He reported that when success prevailed "group sentiment, commonality of interests and objectives" were strong in the group; that success varied with the degree of group solidarity and the rate of social interaction. Finally, Joel and Shapiro (**85**) found a high relationship between patterns of interaction in a therapy session and patterns of interaction of characters in the MAPS Test and TAT stories.

The study by Horowitz, Lyons, and Perlmutter (**76**) indicated that agreement or disagreement with acts in the group is significantly related to one's attitude toward the person perceived as the source of the acts and the person to whom the acts are directed. These attitudes are built up on the basis of agreements and disagreements displayed by the person in response to one's own acts as well as the responses of another to the person's acts.

Again, Norfleet (**121**) related personal liking for a group member, judgment of productive contribution, and actual contribution to a discussion. She found that personal liking, measured by sociometric questionnaire, was poorly correlated with the actual number of contributions made by a person. However, judgments of productive contribution became more highly related to the total number of actual contributions as the period of study progressed. Working from another direction, Rosenthal

and Cofer (**137**) discovered that the nonparticipative behavior on the part of one group member produced measurable effects on attitudes toward the group goal of other group members.

Thus social behavior, values, and motivation are interrelated in a complex whole both within and between people. These in turn affect group properties such as group solidarity, commonality of interests, group satisfaction, etc. The nature of the interrelationships and the mechanisms of influence remain to be worked out.

Studies of Sociometric Choice Patterns

In a variety of the small-group studies already reviewed, sociometric techniques, as devised by Moreno (**119**), have been used as a means of revealing social structure. The sociometric studies to be discussed in this section make the choice pattern itself a focus for investigation. These are mainly correlational studies, direct relationships being sought between sociometric choice patterns and variables of personality.

Sociometric measures have been used in at least two different ways, as status indices and as indices of clique membership. Sociometric status was found to be positively related to job satisfaction in groups of carpenters and bricklayers by Van Zelst (**162**) and to skill by Whyte (**165**), who used bowling teams in his study. Fulton (**55**) obtained correlations of the order of .54 between teammate status, measured by student choices, and skill in volleyball. However, such simple positive correlations were not obtained by French and Zander (**52**), who related popularity of individuals to their production level in industry. In some cases the correlations were positive while in others negative. They concluded, as does Homans (**75**), that status within a group varies more directly with the extent to which a person conforms to production standards set by the group than to the production level itself. Thus by linking these correlational studies more closely to what is known about social interaction processes, better sense can be made of the results.

Hunt and Solomon (**78**) observed a summer camp group of young boys. They found significant correlations between group status and previous experience in camp, athletic ability, generosity, physical attractiveness, orderliness of activity, and lack of egocentricity. With time in camp the correlation between group status and such a palpable characteristic as athletic ability decreased while those correlations between group status and behavioral traits increased. Lemann (**94**) related sociometric overchoosing to personality traits and found that girls with high status were rated as generous, enthusiastic, and affectionate, while girls with low status were rated as stingy, apathetic, and cold. Tagiuri (**158**), using preparatory school boys, related several dimensions of personality to sociometric choice data. He found that the perception of others' preference for oneself is more accurate if one chooses the other than if one does not. Also a person belonging to a religious group exhibits preference for others who belong to the same group. Finally, there were significant differences revealed in the choice patterns of students rated by the school psychiatrist as adjusted and those rated as maladjusted. This relationship between social status and adjustment has also been found by Northway and Wigdor (**122**) as well as by French (**54**).

Thus, the choice of another in a social situation is affected by the

personality characteristics of the other in its overt expression. However, the choice of another is also affected by the personality characteristics of the person choosing and by the properties of the group situation, a conclusion drawn from a study by Maas (**103**) and from Jennings' (**84**) extensive field study in a girls' training school. A theoretical approach which can deal with such a complementarity of factors seems better warranted than the unilinear approaches so often used in experimental design and explanation.

Friendship patterns have also been investigated by the use of sociometric techniques. The general procedure in these studies has been first to identify cliques and then to look for common characteristics among the members. Smith (**145**) analyzed such factors as sex, residence, athletic and nonathletic activities, grades in school, church preference, and father's occupational status as possible variables influencing the choice of friends among high school students. He concluded that students select friends who are in some ways like themselves and that consequently friendship selection might be merely a form of "ego-expansion." However, Bonney (**23**) found little relationship between the same factors in the mutual friendships of elementary school, secondary school, and college students, while Thompson and Nishimura (**161**) concluded on the basis of their study that friendships are formed, not so much because two friends are alike in terms of manifest traits, but rather because each approximates the ideal of the other.

These studies of friendship determinants are similar both in methodology and conflicting results to the studies of marriage mate selection. For example, Hollingshead (**73**) identified two polar theories about the selection of marriage mates. One he called the theory of homogamy that "like attracts like" and the other he called the theory of heterogamy that "opposites attract each other." His data support the theory of homogamy, having used race, age, religion, ethnic origin, and class as his variables. By using marriage mates, a greater degree of control impinges on the data than is the case with the sociometric clique studies. However, since these investigations are tangential to the main body of small-group studies, there seems to be no necessity for further discussion of results here. Nevertheless, a generalized theory of the precise influences operating in the selection of friends or marriage mates remains to be formulated. Perhaps by linking this research more closely with evidence and interpretation concerning primary group affiliations, authority relations, group process dynamics, situational influences, and personality expression, the results will become more cumulative than they have been in the past.

Summary

The study of small groups is flourishing. Promising research techniques are available and others are being devised. These have been used to investigate a variety of substantive problems. The substantive problem areas in small-group study, defined in terms of the independent variables specified in experiments, are classified under five topical headings.

1. The studies which aim to contrast and compare the behavior of groups and individuals show that group discussion processes have an effect on individual behavior in a

wide range of activities. However, this demonstration raises new and troublesome problems concerning the mechanisms by which this influence is achieved and the conditions under which it operates.

2. Under the social structure heading, studies indicate that authority relationships external to the group have important consequences on small-group behavior measured by productivity scores, attitude ratings, quantity of communication, etc. The small group is regarded as a dynamic system of action in the recent experiments which have ingeniously established and changed authority relationships in laboratory settings.

3. Cultural variables, studied within organized groups with a past history of sharing norms or within artificial groups with attitudes induced by experimental arrangement, have been shown to have effects on other small-group behavior such as expression of aggression, absenteeism and turnover, decision-winning, amount of communication, etc.

4. So far, knowledge is meager about the influence of situational variables, such as group task, size of the group, spatial arrangement of members, and communication networks, on group behavior. Such knowledge is vital to every small-group study since the results are conditional on these factors. Too often situational variables have been uncontrolled and unspecified in the design of experiments.

5. The majority of the studies included under the personality-variable heading have to do with leadership behavior. The study of leadership as a social structure variable or a personality resultant has provoked the greatest interest in small-group research. This may be due to many factors, two of which may be a dominant value emphasis in our society or an adaptation to practical demands from the army, business, and industrial organizations. However, the results have not been as substantial as might be expected. Advances in knowledge are anticipated from studies which seek relationships between specific measures of behavior of group members and personality data.

The artificial division into structural components of society, culture, situation, and personality is not to suggest independent function of these variables. The structural-functional design is an exigency in social science research where simultaneous manipulation of all variables is impractical at the present stage of development. Rather, the theoretical approach which seems best warranted in the face of the current evidence is one which views the small group as a dynamic system of action, action determined by a complex of interdependent or interacting factors.

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ALEXANDER, FRANZ, & ROSS, HELEN. (Eds.) *Dynamic psychiatry.* Chicago: Univer. of Chicago Press, 1952. Pp. xii+578. $10.00.

The announcement of this book is likely to spur some scientific minds, prudent though they may be, to a gallop of expectations. It is introduced as the promised fulfillment of a current need: "the most complete picture possible at this time of the fundamental ideas of dynamic psychiatry." The application of psychoanalytic theories to the understanding of neuropsychiatric disorders has never been fully and adequately represented in one concise volume. The title of this book—in which the word "dynamic" has been substituted for "psychoanalytic"—speaks for an intention to do just this and more: to assess the utility of all promising directional concepts and propositions regardless of their ancestry. The title suggests that the editors realize that Freud was not the parent or grandparent of *every* relevant dynamical principle, and that their compilation, consequently, will extend beyond the prescribed limits of a parochial textbook. The list of authors is a dazzle of eminence: some of America's foremost psychiatrists—seven from the Chicago Institute for Psychoanalysis—and, in their company, a distinguished anthropologist and psychologist, Margaret Mead and David Shakow. That Franz Alexander, one of the most imaginative and wide-ranging of analysts, was the guiding light of this enterprise is close to a guarantee of large and unprejudiced dimensions. Finally, there is an inviting table of contents and an agreeable format. Here, surely, is a summation of signals that is more than enough to kick off the salivary reflex and whet one's appetite for the fare between these covers.

The reader's satisfaction with *Dynamic Psychiatry* can be more confidently predicted, however, if he will shut his senses to such signals and, instead, perform a little ritual of negative magic. First of all, he should prepare his cognitive machinery for a book which, like many another, begins and ends with Freud.

As Brosin says in the last chapter: "The system of ideas worked out and presented to the world by one man, Sigmund Freud (1856–1939), has exercised so profound and far-reaching an influence on the minds of men that it may well be ranked among the great new ideas which, like Darwin's theory of evolution, have shaped the course of history." With this judgment and prophecy most of us psychiatrists and psychologists, I presume, would unhesitatingly agree, especially since by this agreement we provide ourselves with defensible ground for a high evaluation of our profession. None could be more deserving of our everlasting gratitude than the man whose penetrating observations and formative reflections have made the once inconsequential disciplines of descriptive psychiatry and academic psychology consequential, and who, in so doing, has provided us with new and dynamic modes of thought for the analysis and clarification of elemental human problems. What could be more timely, if the contemporary world is approximately what it seems to be—*in extremis*, threatened and disoriented, a great tangle of elemental conflicts crying for illumination and decision? Without Freud, today's psychiatrists

and psychologists would have very little, if anything, to say that is applicable in this crisis; but *with* Freud, there is a slight possibility, despite his overshadowing pessimism, that they may contribute to the construction and verification of a healing ideology which, through child training and education, might shape, as Brosin suggests, the course of history.

How Freud's thoughts have influenced or might influence the course of history does not become apparent here, but what *does* become apparent is the exclusive degree to which they have shaped the course of this book, *Dynamic Psychiatry*. According to my count—for those who respect numbers—within the first two pages of each of the sixteen chapters (32 pages in all) Freud's name is mentioned more than fifty times and no other name from Aristotle down is mentioned more than three or four times. There are arrays of facts which show how much social scientists have learned from Freud, but few evidences that Freudians have learned anything or have anything to learn (which is it?) from the other social sciences. This might be taken as one of countless proofs of Freud's towering genius which "doth bestride the narrow world" of psychological theory "like a Colossus," so that "we petty men" can do nothing "under his huge legs," but "peep about," hoping against hope to find an unnoticed little fact or two; and/or this might be taken as one of countless proofs of Freudiolatry, or an Omniscient Father complex, which, if too compelling, would be the nemesis of creativity since, in Whitehead's words, "a science which hesitates to forget its founders is lost." Freudiolatry, as an imprisoner of thought, is nowhere more painfully conspicuous than in the area of motivation (instinct, need, drive), where the analysts' loyalty to their charismatic author confines them to the dramatic, but wholly inadequate, dichotomy of Empedocles (love and hate, libido and aggression, Eros and Thanatos). It is necessary to heap rationalization on rationalization in order to subsume under these two headings the galaxy of expressive and purposive dispositions which constitute human nature—from anoxemia, thirst, hunger, and excretion, the sucking reflex and the maternal drive, the dread of injury, ignominy, and death, the craving for property, power, and prestige, to the mental needs for the creation and expression of comic, aesthetic, scientific, and moral-legal forms. The book is mute in respect to the here-pertinent conceptions of McDougall, Tolman, Adler, Cattell, and others.

Secondly, the prospective reader should prepare his mind for a compilation of more or less independent essays on a variety of topics, rather than an integrated survey of "the fundamental ideas of dynamic psychiatry." Psychoanalytic concepts are expounded in different ways in different places, but no statements of them compare in clarity and distinctness to those of Hartmann, Kris, and Loewenstein. Furthermore, in one crucial area, neither the comprehensiveness nor the organization of this book approaches the level of Fenichel's *Outline of Clinical Psychoanalysis*. Only twenty-three pages (about 4 per cent of the total) are devoted to a systematic account of "neuroses, behavior disorders, and perversions."

Having steeled himself against disappointments of these sorts the reader will be in a position to enjoy and to be greatly instructed by a number of excellent articles. Two

chapters—one by Therese Benedek and the other by Margaret W. Gerard—taken together, provide an admirable picture of normal and abnormal developments in childhood and adolescence as seen through Freudian and Alexandrian spectacles. I, for one, was rewarded by reading the carefully considered sentences of John C. Whitehorn. In the "Principles of Psychiatric Treatment" Maurice Levine presents a clear and concise outline of the tried practices of the therapist and the tendencies in the patient which commonly promote and those which commonly impede the curative process. Alexander is at his best with Thomas S. Szasz in a brief 26-page review of the psychosomatic approach in medicine. In "Some Relationships between Social Anthropology and Psychiatry" Margaret Mead is as interesting as ever; and David M. Levy is perfect, according to my scales, in his informative, judicious, and well-written "Animal Psychology in its Relation to Psychiatry." Altogether there is much substantial nourishment to be extracted from these pages.

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DEESE, JAMES. *The psychology of learning.* New York: McGraw-Hill, 1952. Pp. ix+398. $5.50.

In the preface to this book, Deese tells us that he has tried to bring together "all the divergent interests in learning" within a text that is "by and large empirical in approach," of "strong behavioristic flavor," and intended primarily for college juniors and seniors. This is a fair statement of what he has done. Except for *motor skills*, no major interest is absent; the account is satisfactorily empirical, the flavor objective, and the writing geared to undergraduates in all but a few paragraphs (e.g., in discussing "Current Problems . . . ," pp. 343–353).

In the Introduction, however, he promises a book with a theoretical framework of such "wide application" that "a great many examples of learning will be covered with a relatively economical set of principles and concepts" (p. 3). He leads us to expect that the first third of the book (Chs. 2–7, Reinforcement and Learning, The Nature of Extinction, Stimuli in Learning and the Process of Discrimination, Motivation and Learning, Negative Reinforcement and Punishment, Serial Learning and the Chaining of Responses) will be truly basic to chapters in the second third (Chs. 9–12, Retention, Forgetting, Transfer of Training, and Efficiency in Human Learning) and not unrelated to such later chapters as Thinking and Problem Solving (Ch. 13) or Emotion and Conflict (Ch. 15). In this he lets us down. We get, instead, an unrepresentative and badly edited account of reinforcement theory; a cluster of McGeoch-type chapters on verbal learning one of which (Ch. 8, Factors Affecting Rate of Complex Learning) should have been spread among three others (Chs. 7, 9, and 12), and two of which (Retention and Forgetting) are kept apart on the assumption that forgetting is an intentionally or experimentally induced loss of retention; and a final section of five chapters, four of which (Chs. 13, Thinking . . . ; 14, Learning and the Nature of the Learner; 15, Emotion and Conflict; 16, Physiological Problems in Learning) are little more than 1952 models of their 1940 Hilgard-and-Marquis counterparts. No new systematic relations appear in all this; and some old familiar ones are either absent or seriously blurred.

Generalization, for example, is introduced in Chapter 4. It properly carries the burden of transfer in Chapter 11. In Chapter 7, it accounts for the anticipatory and perseverative errors that produce the serial position effect in Hull's linear maze, and the suggestion is made (p. 141) that it will look after remote associations in rote learning. But, in Chapter 8, the serial position effect in both linear-maze nonsense-syllable learning is *attributed to* remote associations, as well as to the Pavlov-Lepley-Hull inhibitory process (p. 164). Later, in the same chapter, the position effect is cited as an example of intratask interference, which is in turn said to arise from intratask "similarity" (p. 168). Finally, generalization is mentioned but once (p. 198) in discussing retroaction (where "similarity" is again used), and not at all in treating "insightful problem solving" ("a special case of . . . transfer") or concept formation.

The first six chapters will give Deese's readers the most trouble. Here the influence of Skinner and Hull predominates, but the viewpoint offered is that of neither. Reinforcement theorists will be disturbed to find that for extinction to occur an organism must "*perceive* the absence of reinforcement" (p. 62); that "the operational definition of drives . . . will not do for human learning" (p. 98); that negative reinforcement is "painful stimulation for *not* doing something" and is applied to "poorly motivated" behavior, whereas punishment is "painful stimulation for doing something" and is applied to "highly motivated" behavior (p. 111) and that "generalization [in serial learning] could be simply on the basis of the time sense: that is, errors could occur because the subject 'loses his place' " (p. 141). Later on in the book they may also be distressed by the use of such explanatory decoys as "ego involvement" (pp. 178, 180), "set" (pp. 181, 193), and "*Einstellung*" (p. 262). Other readers, even undergraduates, will be irritated by numerous slips that should have been picked up in the editor's office. For example, there is the assertion that the "percentage [*sic*] of conditioned responses becomes more and more frequent as we increase the number of reinforcements" (p. 16); there is the use of "secondary reinforcement" in the last two sentences of page 110, when "negative reinforcement" must have been meant; and there are several cases in which the textual reference to figures (e.g., Figs. 2, 15, 44, and 49) is inaccurate in some detail, trivial or otherwise.

Nevertheless, Deese's book has points of real merit. The narrative marches at a good clip, reader interest is generally maintained (at least *this* reader's interest was), and there is a welcome freshness in the selection of researches reported. Several chapters are of first-rate quality (e.g., the ones on chaining and transfer) and others don't miss the mark by far. For that matter, the whole book points in the right direction and it might have come off successfully if another year had passed before it went to press.

FRED S. KELLER
Columbia University

DAVIDSON, HENRY A. *Forensic psychiatry*. New York: Ronald Press, 1952. Pp. viii+398. $8.00.

Many a lawyer seems to confuse the fields of psychology, psychiatry, and psychoanalysis. In general, he is likely to believe that all psychologists deal with problems of abnormality, or else that they spend their spare

moments chasing white rats through mazes.

Equally confusing is the picture which many a psychologist or psychiatrist has of the field of law. Most psychologists and psychiatrists, along with other nonlawyers, tend to think of all law as being *criminal* law. They also fall into the error of thinking of practically all law practice as involving trial work and courtroom problems.

The distinguished author of the present volume commits neither of these errors. He is well aware of the civil as well as the criminal problems in the field of law. He is conversant with problems of legal psychology which do not center on trial procedures.

Dr. Davidson is Chief of the Program Analysis and Planning Section of the Psychiatry and Neurology Division of the Veterans Administration. His book on forensic psychiatry demonstrates that he has had a wealth of experience in various fields of psychiatry. The present volume gives the reader pretty good insight into courtroom situations so far as the expert witness, especially the psychiatrist, is concerned. At the same time it deals with other important areas as well—problems of competency, malingering, the last will and testament, appraisal of the sex offender, evaluation of personal injury, etc. Essentially this is a manual for physicians and psychiatrists, to serve them as a psychiatric legal guide. Thus it enables specialists in medical and clinical practice to gain a better understanding of the psychiatric aspects of the law. Certainly other professional workers, especially clinical psychologists and social workers, will also find certain chapters of particular use. The chapter on malingering would be of interest to any clinician.

The first half of the book deals with the contents of forensic psychiatry. The latter half describes the tactics of testimony, and this includes some very practical advice to the medical witness as to what to expect and how to be prepared for various courtroom situations. Of particular interest to anyone with an interest in semantics is Chapter 21, The Translation of Technical Terms.

The Appendix is unique. Among other things it contains a legal lexicon for doctors, so that they will have some notion of what is meant by such terms as contributory negligence, exemplary damage, hearsay testimony, and self-serving statements. The psychiatric glossary for lawyers, in turn, gives the man of law some impression of what the psychiatrist has in mind when he uses such words as ambivalence, compulsion neurosis, etiology, psychopathic, and the like. It is unfortunate that a psychologist is defined as "a nonphysician trained in the understanding of mental mechanisms"; and it is equally unfortunate that no definition is included of a psychiatrist.

A significant contribution of the book is a detailed proposal for a model act of the certification of the mentally ill. This should be useful in many different jurisdictions.

It is difficult to find fault with this work. It is easier and more accurate to say that one can learn a good deal from studying it.

STEUART HENDERSON BRITT
Needham, Louis and Brorby, Inc., Chicago, Ill.

RIESEN, AUSTIN H., & KINDER, ELAINE. *The postural development of infant chimpanzees. A comparative and normative study based on the Gesell behavior examination.* New Haven: Yale Univer. Press. 1952. Pp. xx+204. $5.00.

This volume is one of a projected series to be published in connection with the Infant Studies Program which has been in progress at the Yerkes Laboratories of Primate Biology since 1939. Chimpanzee infants have been isolated from their mothers and reared in an experimental nursery under carefully controlled conditions while various aspects of their development have been charted. Riesen and Kinder report on postural development in 14 apes during the first year of life.

The tests were selected from the Gesell and Thompson examination. They included the postural situations (supine, prone, pulled to sitting, sitting, and standing) and some data on the grasping response obtained in the Rod situation. The evidence is presented in considerable detail (18 tables and 117 graphs). At each step, the performance of the chimpanzee is compared with that of human children of comparable age as determined by Gesell and Thompson.

The development of a postural repertoire in human and chimpanzee infants is quite similar. In both species the individual gradually gains control of the head, loses some neonatal reflexes, "learns" to roll over, to sit, and to stand and walk upright. Independent movements of arms, legs, hands, and feet change from month to month. Developmental similarities between chimpanzees and humans are closer for head and general body postures than for postures of the arm, leg, or hand. The ages of appearance of the developmental items average one-third earlier for chimpanzee infants in those cases where a valid basis for comparison is available. On a few items the human infant shows an earlier age of development than the ape.

The investigators devised a postural schedule for chimpanzees, consisting of 60 selected items which showed a split-half reliability of .95. When this schedule was applied to infants left with their mothers or reared in a human home these animals were found to fall within the range of the nursery group. In conclusion the authors critically examine several theories and concepts of behavioral development and find that some of these are not supported by their data. Other concepts, such as that of the assumption of cortical control, that of alternation of extensor and flexor dominance, and the theory of of correspondence, are in agreement with Riesen and Kinder's findings.

This extremely detailed and at times highly technical analysis of postural development is a scholarly achievement and an important addition to the literature on genetic psychology. With the exception of Marion Hine's monograph on the macaque there is no comparable work of equal calibre. It is to be hoped that subsequent reports growing out of the Infant Studies Program will meet the high standards set by this opening publication in the series.

FRANK A. BEACH
Yale University

GORLOW, L., HOCH, E. L., & TELSCHOW, E. F. *The nature of nondirective group psychotherapy.* New York: Teachers College, Columbia Univer., 1952. Pp. xii+143. $3.25.

This book is a report of statistical analyses of data secured in nondirective group psychotherapeutic sessions conducted at Columbia University. The three authors served as therapists in the study. Its primary value is as a pioneering research project which demonstrates the applicability of research analyses to some aspects of group psychotherapy. However, the conclusions reached were based on data secured from only seventeen

clients, all graduate students, and only three therapists. Verification of these findings by other therapists working with a less homogeneous group of clients must precede their acceptance.

About forty conclusions are stated by the authors, of which the following are examples: (1) It is possible for therapists to be nondirective in 98 per cent of their statements and have clients improve. (2) It may be possible to predict in early sessions which clients will or will not benefit from the nondirective approach. (3) The frequency of positive statements increases in a manner similar to the increase which occurs in individual therapy, but negative statements increase during the early sessions, then decrease during later sessions. (4) The least-profited clients tended to treat nondirective statements by the therapists as "threats" which put them on the defensive. (5) The most-profited clients tended to be more nondirective in dealing with other clients from the first session on. (6) Amount of participation by clients did not correlate with amount of improvement.

There are several methodological and technical weaknesses in the study. First, much is made of comparison of most-profited and least-profited clients. The authors used subjective criteria for evaluating improvement and present inadequate evidence of validity of these criteria. However, many of the conclusions are based on the assumption that the criteria were valid. Second, the authors confused nondirective, directive, interpretative, and critical statements in their classification of client responses. For example, they classed approval, encouragement, and reassurance responses as nondirective, and opinion, counteropinion, requesting client to elaborate, persuasion, suggestion, advice, interpretation, and deflection as "neutral." This unfortunate classification may invalidate several of their conclusions; their generalizations about nondirective client statements actually include some directive categories, and generalizations about evaluative and interpretative statements actually refer to negative criticisms. Third, there is a dearth of original data in this book. Only three or four quotations of client statements are presented in the entire book. Consequently, the face validity of the authors' interpretations and classifications has to be assumed or challenged by the reader. Finally, they appear at times to overgeneralize and to conclude that differences are significant or not significant without applying tests of significance.

WILBUR S. GREGORY
The University of Redlands

SCHEIDLINGER, SAUL. *Psychoanalysis and group behavior.* New York: W. W. Norton & Co., 1952. Pp. xviii+245. $3.75.

This book is an attempt to summarize the "orthodox Freudian" point of view regarding the dynamics of group phenomena. As the author notes, there have been no scientific researches which have attempted to test Freudian hypotheses in this area, so the source materials for this book are the theoretical concepts and hypotheses of Freud, Slavson, Fenichel, Redl, Money-Kyrle, and others. The author merits special commendation for stressing the hypothetical nature of the principles presented and the need for research (he devoted the entire seventh chapter to these points)—an emphasis which makes this book somewhat unique among psychoanalytic writings.

The first five chapters are a detailed but repetitious and wordy survey of psychoanalytic literature dealing with group phenomena. Chapter 6 is a recapitulation of them, and in it the author does an excellent job of stating the concepts concisely and in a manner that makes it easy to contrast or integrate them with non-psychoanalytic approaches to group behavior. Although he mentions the Freudian principle that the family is the prototype of all groups and that leaders are father-substitutes, he gives equal emphasis to conscious and unconscious emotions, the functions of identifications, regression in groups, libidinal and aggressive motivations of group behavior, the benefits to the individual of membership in groups, factors which enhance or endanger group cohesion, the roles of leaders, and group climate. Although many of these principles are neither original with, nor limited to, the psychoanalytic viewpoint, all of them must be included in any summary of the psychoanalytic viewpoint.

In the last three chapters the author attempts to show the need for applying psychoanalytic principles to sociology, education, and group therapy. Although his theories regarding sociological research and his philosophy of education are stimulating, they are by no means original with, nor dependent upon, psychoanalytic theory. The discussion of group psychotherapy is of interest because it presents the various analytic viewpoints regarding the dynamics of group psychotherapy.

WILBUR S. GREGORY
The University of Redlands

VERNON, PHILIP E. *The structure of human abilities.* New York: Wiley, 1951. Pp. 160. $2.75.

In little more than a decade the number of studies attempting to analyze the structure of large test batteries by use of factor analysis methods has mushroomed tremendously. As an aftermath to such vigorous activity it was inevitable that someone should try to draw a major segment of this somewhat contradictory, fragmentary array of studies into a closely knit, coherent presentation. That responsibility for undertaking such a task should be assumed by a prominent British psychologist is indeed a fortunate event for the American psychologist, who is usually too busy trying to keep up with the research of his associates to pay much attention to activities abroad.

In attempting to accomplish this objective, Vernon has done an unusually impressive job of thoroughly covering research pertinent to an understanding of human abilities. Frequently this has involved the reworking and reinterpretation of the results of many studies in order to fit them into a consistent pattern. His book, though small, is well organized and meaty, reading very much like a critical review article in the *Psychological Bulletin.*

While trying to be objective Vernon makes it clear from the beginning that he holds closely to the hierarchical group-factor theory of mental organization first put forward by Burt. Spearman's *g* permeates everywhere as a general factor distinguishable from two main group factors, the verbal-numerical-educational (referred to as the *v:ed* factor) and the practical-mechanical-spatial-physical (called the *k:m* factor). If an analysis is sufficiently detailed, these two major group factors can be split into numerous minor group factors which are of little importance according to Vernon. Although a major portion

of his book is devoted to a discussion of such so-called minor group factors, the author believes that little can be done to improve vocational guidance and selection after taking into account *g*, *v:ed*, and *k:m*. This is in sharp contrast to the prevailing American approach of designing test batteries for measuring a number of relatively independent ability factors from which an individual's pattern of factor scores can be derived and used in vocational guidance.

Vernon takes American factor analysts to task on several counts. Although the British factorists in the past may have been overcautious in extracting too few factors from a correlation matrix, most Americans have been far too indulgent, often extracting a dozen factors from a single matrix. While this is no doubt true, it should be pointed out that insignificant factors are frequently dropped from the analysis at a later stage when the original centroid factors are rotated into a more meaningful structure.

The point of greatest disagreement, however, concerns the role to be assigned a *g* factor. While British writers make *g* as large as possible, positing group factors only when the first centroid residuals require it, Americans tend to introduce it as a second-order factor or else rotate it completely out of the picture. Vernon argues that since *g* is so much larger than all other factors put together in *unselected* populations it is foolish to belittle it. Because most British factorists deliberately maximize *g* by choosing a set of reference axes which is favorable to their preconceptions, it is not surprising to find that *g* explains most of the common variance in many test batteries. But this is largely a matter of choice and the same correlation matrix can be explained just as adequately (and in the opinion of many factorists more meaningfully!) by a rotation of the reference frame which minimizes *g* and results in multiple-group factors.

Seven main arguments have been put forth in the Appendix to support the writer's stand that his hierarchical group-factor theory is superior to any other. While several good points are made and will prove stimulating to the reader, it is doubtful that they are sufficiently crucial and persuasive to convince most factor analysts who have found the multiple-factor methods of Thurstone satisfactory.

This is the clearest, most consistent, and well-documented presentation to date of the hierarchical group-factor theory which dominates the British scene. Regardless of one's personal predilection concerning factor analytic procedures, careful study of this little book will prove highly stimulating and informative.

WAYNE H. HOLTZMAN
University of Texas

HIRSH, IRA J. *The measurement of hearing*. New York: McGraw-Hill 1952. Pp. ix+364. $6.00.

The purpose of this book is to provide background material in acoustics, electronics, psychophysics, and psychoacoustics for those who are entrusted with clinical audiometry. No previous technical knowledge is required to read it, the matter of clear nonmathematical definitions being given especial care. It is excellent in the initial training of individuals who must be assigned to clinical audiometry but who bring no background in the psychology of hearing and have had no experience with the psychophysical method. It has more limited value for those who can be asked to read chapters on sound and elementary electronics in physics

texts, Guilford's *Psychometric Methods*, and the chapters on acuity, differential sensitivity, and loudness in *Hearing* by Stevens and Davis.

The book is not, however, a simple manual in the techniques of audiometry. After perusing it, the reader would not at all know how to go about taking an audiogram. This is not bad—there are are plenty of manuals and texts on that topic, though standardization is yet to come. But it emphasizes the fact that this particular book is a serious attempt to examine the psychological backgrounds of collecting absolute intensive limens, and the psychological, not the clinical, meaning of those limens once collected. No other source does just this, and the book very definitely fills a need. It may surprise a psychological audience that their contributions to the psychophysics of threshold determination and to sensory scaling have not infiltrated more widely into the new profession of audiology. But the reviewer has lectured to graduate students in that field none of whom had thought of defining their stimulus in physical terms of sound pressure level in a closed coupler or in the ear canal itself; they were content to accept the hearing loss dial-reading on the audiometer. And there have been applications for employment from doctorates in audiology who could not attempt a definition of the method of limits. It is clear that the profession of audiology would be much better grounded in audiometry, at least, if this book were required reading.

As a reference text in the psychology of hearing, the book has some additional values. There is an excellent chapter on loudness and the recruitment of loudness, one on binaural hearing and bone conduction, and one on masking and fatigue. These, quite the best chapters as reference, give a good deal of up-to-date primary source material. No more errors are committed than are unavoidable in making generalizations from research material for clinical use. The book is carefully built with ample subject and name indexes, glossary, and appendices of audiometer specifications and several word lists for speech reception testing. It is a pleasure to recommend this book to audiometricians, and to psychologists doing research in hearing who want to explore the relation of their work to the clinic.

J. DONALD HARRIS
U. S. Naval Medical Research Laboratory, New London, Conn.

ASHBY, W. ROSS. *Design for a brain.* New York: Wiley, 1952. Pp. ix+259. $6.00.

Perhaps the future historian will label the psychologists of today the "model builders." In any event, we are experiencing great efforts to create models that may prove more fruitful in understanding behavior than our more mundane empiricism of the past. Ashby's book is another of these current attempts to develop a model of the brain as the principal organ of behavior.

Ashby, like the others, uses the machine as his model but his model has the merit of being a generalized machine, not a servo-mechanism nor a computer. Indeed, Ashby's model is really a mathematical one. He conceives the brain to be a system of variables with certain functional properties. He only uses a machine to illustrate these properties, not as the analogue of the brain.

To understand the model, one has to read the book carefully and digest each of many steps that are essential

to his argument. Ashby presents them in nonmathematical form in his main text, leaving the proof to an appendix. The skeleton of the argument is this: The brain is a dynamic system of variables, a system in which values of variables are constantly changing. At any one moment these values constitute a state, and the system moves from one state to another. The path established by successive states is the *line of behavior*.

To explain behavior we must assume the system to have certain properties. For one thing, the system is *absolute;* given any one state, all lines of behavior following that state are equal—it is a completely state-determined system. It is also a *stable* system, for its variables are so interrelated that they cannot exceed certain limits; it has feedback in it that makes it a homeostatic system. It is a system which at times enters *critical states* that produce relatively sudden changes in certain of its variables, so that its variables describe *step functions;* like a thermostat, one variable suddenly changes when another reaches a certain value. Critical states and step functions combine to endow the system with *ultrastability*, that is, with preferences for states that do not lead to critical states. Finally, many of the variables in the system are *part functions* of other variables; that is, they are sometimes dependent on, and sometimes independent of, each other.

Assuming these and other mathematical properties of the brain, Ashby attempts rather successfully to deal with a wide variety of phenomena including physiological homeostasis and survival, adaptation, learning, and goal-seeking. He even seems to prove that learning and memory must not be localized very well in the nervous system but rather dispersed, as one might conclude from Lashley's work.

This reviewer is not at all confident of the outcome of all of this model building. If it is a good thing, he prefers Ashby's formal mathematical approach to the mechanical and electrical analogies of such model builders as McCulloch and Wiener. Ashby, moreover, has worked out a general, comprehensive model, in contrast to the piecemeal approach of many model builders. His key concept, that of ultrastability, is a feature not existing, or at least not well developed, in other models and it seems to have potential power to explain the knottier problems of motivation, purpose, and complex mental functioning. The book, therefore, contributes importantly to a mathematical formulation of behavioral phenomena.

C. T. MORGAN
The Johns Hopkins University

THURSTONE, L. L. (Ed.) *Applications of psychology.* New York: Harper, 1952. Pp. x+209. $3.00.

Any book which is the joint product of several authors will necessarily display the diversity of viewpoint and styling which are characteristic of those who have participated. *Applications of Psychology* is a good example.

Prepared in honor of the seventieth birthday of Walter V. Bingham, the contributors were chosen from among the many who had been associated with Dr. Bingham either at Carnegie Institute of Technology or during his two periods of service as a military psychologist. These are representative of the many current areas in applied psychology.

The topics covered range from somewhat theoretical discussions of creative talent through the validity

of the medical and life insurance scales of the Strong Vocational Interest Blank to the clinical evaluation of Harvard undergraduates and the use of the clinical method in the selection of employees.

The importance of the volume would seem to lie chiefly in its tracing of the variety of present-day practices to their early beginnings and in its documentation of the effects of Bingham's influence on the development of many phases of professional psychology.

As a testimonial to the respect and admiration accorded to Bingham, it is an effective document. As a communication in the field of psychology, it leaves much to be desired.

GEORGE K. BENNETT
The Psychological Corporation

WOLFF, W. *The dream—mirror of conscience.* New York: Grune & Stratton, 1952. Pp. vi+348. $8.50.

The sense of diffuse dissatisfaction with the first section of this book (dream and history) is perhaps the consequence of its inevitably episodic character; inevitably, because the author could naturally hit in the most fleeting fashion only the highest of the high spots in presenting Greek, Roman, and Medieval treatments and views of the dream as well as Egyptian, Babylonian, and Biblical accounts. With chapters running about a page and a half to two pages one can hardly be left with any other feeling than that something is unfinished. However, any other treatment would have required a much larger book and perhaps have changed its slant and intent.

With respect to a few minor disagreements, in the first place it might be that the author takes too literally Freud's complaint that "nothing new or valuable" has been written about the dream since 1900. One might grant the stamp of genius almost to all of Freud's works and yet recognize that for him the "value" of a theoretical position was measured, to some extent, by the degree to which it was in agreement with his own. Whether one characterizes Stekel's analysis of dreams as "new" or "valuable," it was certainly *different.* My impression, too, is that Wolff emphasizes too little the distinction that since has been made between Freud's view that the dream tapped the "archaic" in the personality and the view that the "higher" facets function in the dream. The latter case has been strongly argued by Fromm, whose book is not listed in Wolff's. Thus, Wolff's distinction between "the forces that drive man" and "the man using these forces" (p. 280) is certainly not a new one. Indeed, unless I am in error, this differentiation is expressed in the distinction between the "old" or earlier psychoanalysis and the more recent "ego psychology."

Not withstanding these minor criticisms, Wolff's book is stimulating, provocative, and rewarding. There were two outstandingly significant notions. The "theoretical" one is perhaps best expressed in Wolff's own words: "the dreamer uses his dream activity to solve this conflict of needs which conscious reasoning could not solve because the conflict usually involves antagonism between emotion and reason. Thus, a third agency is needed to decide this struggle. This is conscience . . . the dreamer . . . confronts his thoughts with his values and brings them before the mirror of his conscience" (p. 275).

The second major contribution is a proposal for a method of dream analysis or "synthesis." Criticizing

orthodox analytic procedure, in that "psychoanalysts only pick out those dream images which they consider the most significant" (p. 189), Wolff proposes that the psychotherapist "should use *all* the dream images" (p. 190). Further, "we must eliminate not only our personal preferences and leading ideas in a dream interpretation but also those of the dreamer himself. If we give him the dream images as stimuli for his associations in the sequence of the dream, he may pattern his associations according to this sequence. In order to prevent such a preconceived arrangement we give the dream images as stimuli in a sequence different from that of the dream. Moreover, in order to free his associations, the dream stimuli should be given a few days after the dream was told in such a way that the dreamer will not necessarily recognize the words as coming from his dream" (p. 190). For the experimental-minded clinician, this proposal is worthy of investigation.

WILLIAM SEEMAN

Mayo Clinic
Rochester, Minn.

WHITE, ROBERT W. *Lives in progress.* New York: Dryden Press, 1952. Pp. ix+376. $2.90.

This is an important book. It is a valuable addition to the slowly growing shelf of books that provide raw material of psychology suitable for undergraduate students. The book will be welcomed by psychology teachers who yearn for freedom from the tyranny of textbooks. Approximately 50 per cent of *Lives In Progress* is devoted to the life histories of three young adults whom White and his associates studied intensively when the subjects were college students and studied again five to ten years later. The remainder of the book is an exposition of methods of personality study and an analysis of these lives in terms of White's views of personality development. In the reviewer's opinion, the analysis provides an unusually adequate basis for teaching current theories of personality to undergraduate students; its chief virtue lies in its comprehensive and balanced coverage. *Lives In Progress* provides a basis upon which an instructor can develop his own brand of personality theory. Since the case histories are presented with sufficient detail and absence of bias, they can be used to exemplify other viewpoints and emphases than those of the author.

The most obvious use of this book is in undergraduate courses in personality. However, the reviewer has used it very successfully in general introductory psychology, along with a volume presenting another kind of unanalyzed psychological data (Barker and Wright's *One Boy's Day*). These two books freed the instructor to be a teacher of psychology rather than a textbook commentator. *Lives In Progress* has high interest for students; the reviewer found that some students read the book in one sitting and many did so during the first week of the course. This attraction is apparently due partly to the intrinsic interest of the life histories, but also to the fact that White is an excellent writer.

Although the author apparently considers *Lives In Progress* to be largely a teaching resource, it is the reviewer's opinion that it, along with such volumes as Blos' *The Adolescent Personality* and Davis' *Children in Bondage*, is a significant contribution to the small stack of normal lives available in published form to students of personality. The scientific value of these case studies will not

diminish with the passage of time. They may indeed turn out to be important scientific documents a century hence. One can easily understand the historical and psychological interest an 18th or 17th century *Lives In Progress* would have today.

ROGER G. BARKER
University of Kansas

FERGUSON, LEONARD W. *Personality measurement.* New York: McGraw-Hill, 1952. Pp. xiii+457. $6.00.

A volume devoted to the special problems of personality measurement has been needed for a long time, but Ferguson fills only a portion of this gap. A definition of personality is lacking, which would serve as a focal point for the measurement material. Many problems in the logic of measurement are sidestepped, such as the extent to which the addition of item-responses on interest inventories, questionnaires, and projective devices gives us scalable material. Ferguson's book, therefore, must be thought of primarily as a collection of interesting and useful facts about the reliabilities and validities of numerous instruments intended to be measures of aspects of personality.

The first chapter of the book is well calculated to stir the reader's enthusiasm. A case of a college student suicide suggests that we may get interesting clinical studies on validity; a discussion of group differences in personality (national, racial, etc.) hints of measurement problems in cross-cultural comparisons; and three pages on morale whet an appetite for a critical analysis of morale measures. Unfortunately, most of these expectations are unfulfilled; cultural group differences in personality are not mentioned again, and morale studies are given only casual attention.

There is a good deal to be said in favor of Ferguson's arrangement, starting with problems of interest and attitude measurement, which makes it possible to deal with simple concrete materials before getting into some of the more complex devices. It is questionable, however, whether these two topics merit 116 pages (or 25 per cent of the volume) compared to the treatment given to projective devices in 35 pages (or 8 per cent of the book). The suggested imbalance in Ferguson's presentation is further illustrated in the treatment of the projective tests. The chapter on the Rorschach test gives a protocol of a fairly interesting case, and discusses some of the mechanical aspects of scoring. However, at no point is the interpretation quoted; that is, the description of the total personality, which is presumably the main diagnostic claim of the Rorschach approach. Even on the level treated by Ferguson (the statistical analysis of specific response determinants), the work of such investigators as Wittenborn and Cronbach has been ignored. And just why the study of sales managers by Kurtz should be considered evidence on the validity of the Rorschach is hard to see.

The sections on objective, questionnaire-type devices are the best in the book. They summarize data on an enormous number of different inventories, from the Woodworth Personal Data Sheet to the Minnesota Multiphasic Personality Inventory. And information on method of development, reliability, group norms, and validity studies is offered for each of these tests. There is lacking, however, a discussion of such logical problems as what kind of scale results when we simply add the number of "diagnostic" answers. What one also misses is an incisive discussion of just what would constitute

substantial validation. This is a difficult assignment, but it cannot be evaded legitimately in a volume on personality measurement.

This book should be useful as a text for a graduate course in personality measurement, if it is liberally supplemented by journal articles or other sources, and if the instructor provides some balance to the overemphasis on empirical findings and on questionnaire-type tests. The treatment is clear and straightforward; the style is readable without being overpopularized. It will certainly help to give students a model of organization for data on development, reliability, and validity of any test device. They should also learn a good deal about appropriate applications of statistics to defined measurement problems.

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VERNIER, CLAIRE MYERS. *Projective drawings.* New York: Grune & Stratton, 1952. Pp. v+168. $6.00.

This is a well-organized collection of drawings made by persons of a variety of clinical diagnostic groups (the psychoses, neuroses, and brain-damaged) and includes the productions of normal subjects as well. The author offers this book primarily as a teaching tool in courses on projective techniques, and for this end it should be most valuable. Cases were selected for which the clinical diagnoses were well defined and the drawings were then collected to see the extent to which they reflected the already known symptoms. In surveying these productions it is certainly evident that the "Draw-A-Man" test can be valuable in capturing graphically some personality characteristics. What is also strikingly evident, however, is the tremendous overlap of so-called clinical signs from one diagnostic group to another, especially into the normal group. This should allow for considerable humility and care in the use and interpretation of the test until a more standardizing system of dealing with the data can be developed. Vernier's data may present a significant step in this direction. It is important to recognize, however, that most of the observations made about the drawings by the author are ex post facto, which sometimes get her into trouble. In one instance drawings made by a paranoid patient (Figure 2), in which the figures both had small heads, were interpreted as follows: "minimal head emphasis is of interest in view of the patient's lack of either intellectual aspirations or control mechanisms" (p. 6). However, in a drawing made by another paranoid schizophrenic (Figure 4) there is also a small head which is not mentioned; instead, the midline emphasis or double belt line is considered to be a reflection of the patient's emphasis on intellectual controls. There are a large number of similar examples. In general, however, this book should be of considerable value for the teacher of projective techniques, for the practicing clinician, and in suggesting further research with this test.

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